THE AUGUST SCIENTIFIC MONTHLY

EDITED BY J. MCKEEN CATTELL

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THE SCIENTIFIC MONTHLY

AUGUST, 1936

NUTRITIONAL IMPROVEMENT IN HEALTH AND LONGEVITY

By Dr. HENRY C. SHERMAN

MITCHILL PROFESSOR OF CHEMISTRY, COLUMBIA UNIVERSITY
RESEARCH ASSOCIATE, CARNEGIE INSTITUTION OF WASHINGTON

THIS article is essentially a report, interpretive rather than technical, upon the investigation of the relation of food to health and to length of life, which is being conducted at Columbia University with the cooperation of the Carnegie Corporation of New York and the Carnegie Institution of Washington. This is, of course, but one among several Carnegie researches in nutrition.

Practically throughout its history, the Carnegie Institution of Washington has supported nutritional research: in the energy aspects, first by grants in aid of the work of Atwater and Benedict at Wesleyan University, and then by the establishment of its own Nutrition Laboratory in Boston under the direction of Dr. Francis G. Benedict; in the aspects having to do chiefly with the proteins and vitamins, by liberal grants in support of the work of Osborne and Mendel at Yale and the Connecticut Agricultural Experiment Station; while grants in aid of research upon enzymes at Columbia contributed toward the working out of the chemical natures of substances important to the nutritional process both as protein derivatives and as catalysts of the body's chemical reactions without which these would not run fast enough to support such a rate of energy exchange as is essential to the higher forms of life.

ADEQUATE AND OPTIMAL DIETS

The work now to be summarized is a joint outgrowth of these earlier Carnegie researches and of the nutritional aspects of certain food supply problems of the world war (Figs. 1–3). One of these problems was: What minimum proportion of milk suffices so to supplement or balance a maximum proportion of wheat that the mixture will constitute a nutritionally adequate diet?

It was found that a mixture of five sixths ground whole wheat and one sixth dried whole milk, with table salt and distilled water, which we shall call *Diet A*, was adequate in that it supported normal growth and health with successful reproduction and rearing of young, generation after generation. Yet when the proportion of milk was increased, to constitute *Diet B*, the average results were better.

Diet A, then, is adequate but not optimal; Diet B is better and probably capable of still further improvement. We have here a nutritional improvement upon a dietary which was already ade-

quate. In the averages of sufficiently large numbers of cases the evidences of nutritional improvement are measurable in terms of several criteria of increased vitality at different stages of the life cycle; and the measured differences are such as to show that, within the range or zone of normal nutrition and health, there is a rather large area between the merely adequate and the optimal. It is the exploration of this area with which we are here concerned.

The Experimental Animal

The experimental animal chiefly employed has been the rat; because, as had been shown by the work of Osborne and Mendel and of Folin, the chemistry of its nutrition is so closely similar (in nearly all respects) to that of the human being.

Chemically, the only way in which this experimental animal has been found to differ materially from ourselves is that we are much more dependent upon, and responsive to, the vitamin C value of our food. Thus the opportunity for nutritional improvement is similar on the whole in the two species; but where they differ the responsiveness of the human organism is the greater. Hence definite scientific evidence supports the view that the laboratory findings need not be discounted when applied to human prob-

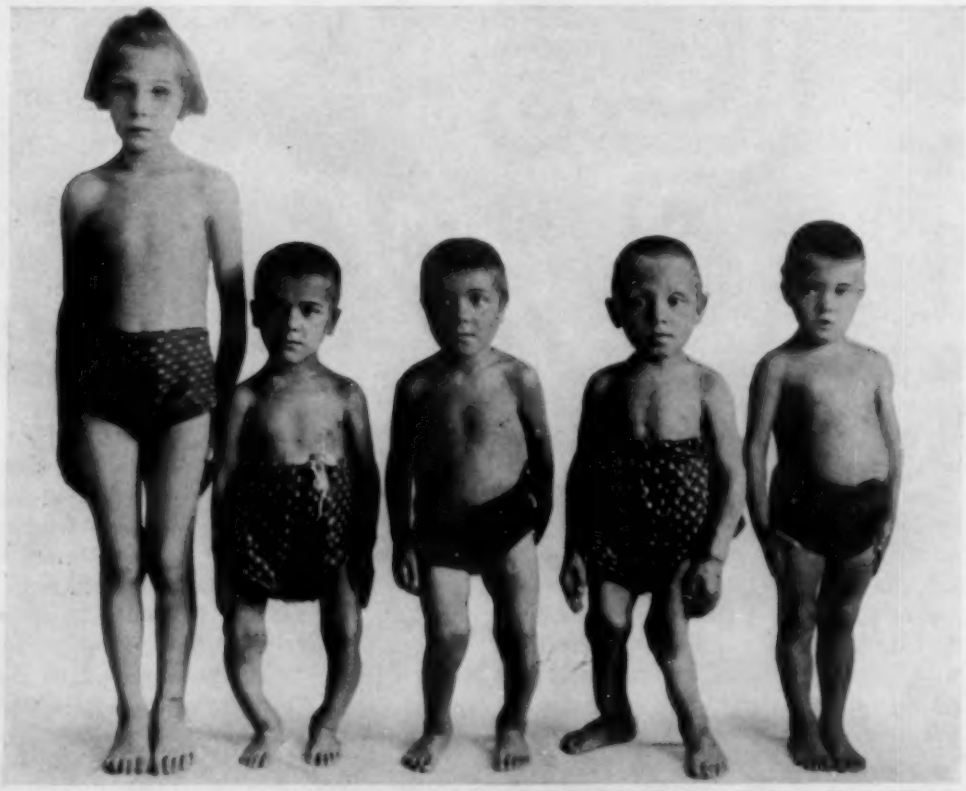

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FIG. 1. EFFECT OF DEFICIENT DIETS.

Five Austrian children, all seven years of age. With four of them such food shortages as lack of milk and eggs markedly influenced growth and development.

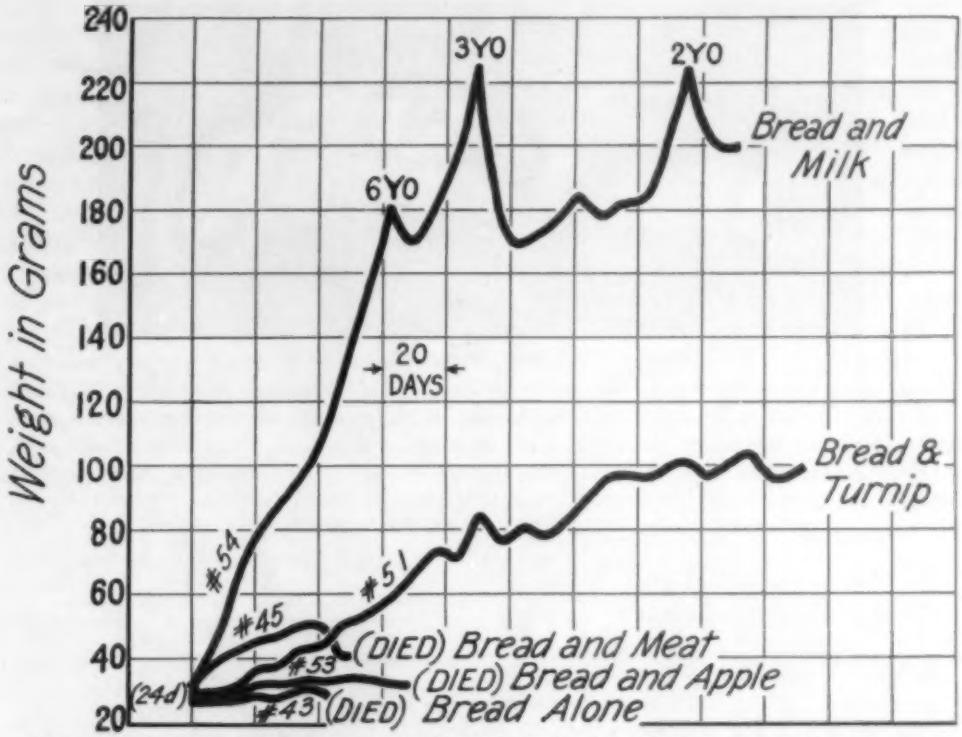

FIG. 2. RESULTS ON RATS OF A DIET OF BREAD ALONE OR OF BREAD WITH ONE OTHER FOOD.

AS SHOWN HERE, MILK PROVED ESPECIALLY EFFECTIVE IN NUTRITIONALLY "BALANCING" BREAD. IN EACH OF THESE EXPERIMENTS WITH BREAD AND ONE OTHER FOOD, THE BREAD FURNISHED FOUR FIFTHS AND THE OTHER FOOD ONE FIFTH OF THE TOTAL FOOD CALORIES.

lems; more probably they are considerably *within* the truth as to the responsiveness of human health to nutritional improvement.

The fact that the rat, while resembling us so closely in its chemistry, yet runs through its normal life cycle in one thirtieth the time of a human life adds, of course, very greatly to the facility with which this deputy can be employed in the experimental study of our nutritional problems. If it were possible to observe human subjects under controlled conditions through entire life cycles for successive generations, centuries would be required to cover the ground covered by the laboratory work of the decade or so which we are here reviewing. Moreover, these laboratory animals are sufficiently modest in their demands for space and cost of living to permit of the use of large enough numbers to justify confident statistical interpretation of findings.

IMPROVEMENT RESULTING FROM CHANGE OF DIET

Several differences in degree of nutritional welfare and resulting health, within the range of what is accepted as normal, have now been thus established.

The first of these differences which we observed was that, in the launching of a new generation, Diet B supports the

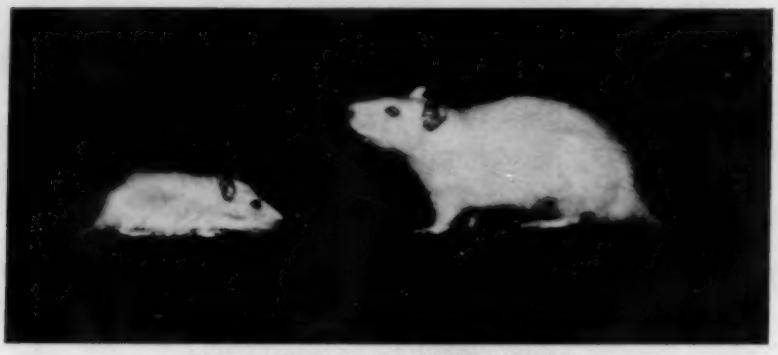

FIG. 3. FROM THE SAME LITTER.

THESE TWIN SISTERS HAD BEEN OF THE SAME SIZE AND EQUALLY HEALTHY AND VIGOROUS AT THE BEGINNING OF THE EXPERIMENT SUMMARIZED IN FIG. 2; THEN DURING THE EXPERIMENTAL PERIOD ONE WAS FED BREAD AND APPLE, THE OTHER BREAD AND MILK. THE LATTER MADE GOOD GROWTH AND APPEARED HEALTHY AND NORMAL IN EVERY WAY. WITH THIS LIMITED PROPORTION OF MILK, HOWEVER, IT WAS ONLY WHEN WHITE BREAD WAS REPLACED BY WHOLE WHEAT THAT THE DIET BECAME ADEQUATE FOR THE SUPPORT OF SUCCESSIVE GENERATIONS.

mother better than does Diet A, and at the same time gives the young a better start (Fig. 4).

Strictly quantitative experiments with large numbers of such families continuing through successive generations and covering (of late, at least) the entire life cycle of each individual, showed that this same improvement of an already adequate diet (1) expedited growth and development, (2) resulted in a higher level of adult vitality as shown by several criteria, and (3) extended the average length of adult life, or improved the life expectation of the adult. Data on the first and second of the three phases just mentioned have been summarized in a previous report,[1] while the data for length of life are shown in Table I.[2]

The increase in average length of adult life here found would correspond to an extension of the long-standing human-adult life expectation of 70 years to 77 years instead. Obviously many cases of people living to the age of 77 fall within

[1] H. C. Sherman and H. L. Campbell, *Jour. Biol. Chem.*, 50: 5–15, 1924.

[2] H. C. Sherman and H. L. Campbell, *Proc. Nat. Acad. Sci.*, 14: 852–855, 1928; and *Jour. Nutrition*, 2: 415–417, 1930.

TABLE I

SHOWING INCREASE IN LENGTH OF ADULT LIFE THROUGH IMPROVEMENT OF AN ALREADY ADEQUATE DIETARY

	On Diet A		On Diet B		
	Number of cases	Average length of life in days	Number of cases	Average length of life in days	Difference of length of life in days
Males..	135	571 ± 8.0	124	635 ± 8.5	64 ± 11.7
Females	196	603 ± 8.0	163	669 ± 7.8	66 ± 11.2

the familiar range of normal experience. Yet inasmuch as previous improvements in the average length of life have been so closely confined to the lowering of *early* death rates as to leave the average length of *adult* life unchanged, the possibility of extending this *average* by nutritional improvement is of interest from several points of view.

It is all the more significant because of the fact that, in these experiments, development is expedited and senility deferred in the same individuals, so that what, for lack of a better term, we may call "the period of the prime" is extended in greater ratio than the life-cycle

itself. Thus in typical cases, such as are illustrated by Fig. 5, the same degree of incipient senility which is reached by normal individuals on Diet A at an age corresponding to about 65 years in the human life is deferred on Diet B to an age corresponding to 75–80 years.

It need hardly be said that on both diets a considerable proportion of individuals, as in human experience, die natural deaths before the attainment of these ages. Naturally also, such nutritional improvements as we are here considering lower the death rates of the young as well as of the middle-aged and old, so that the life expectation at birth is improved even more than is that of the adult.

Effect of "Protective Foods" Recognized

There are evidences of several kinds that in direct human experience this same sort of improvement of food supplies—in general terms, the taking of a larger proportion of the needed calories in the form of what McCollum named "protective foods"—acts to support superior development in children and a greater number of years of "positive" or "buoyant" health in adults. The historical and field evidence collected by McCollum and Simmonds[3] and the clinical data of Langstroth[4] together indicate very clearly that the onset of senility is deferred, or the incidence of the so-called degenerative diseases decreased, when the so-called protective foods are given a more prominent place in the dietary.

The idea of such a shift in the relative proportions in which different foods shall be used is already beginning to spread far beyond the laboratory and the lecture hall. The Secretary of Agriculture writes in an official report: "The goal is optimal nutrition"; the president of the American Medical Association says in his official address that science

[3] E. V. McCollum and N. Simmonds, "The Newer Knowledge of Nutrition," 4th ed. (Macmillan), 1929.

[4] L. Langstroth, *Jour. Amer. Med. Assoc.*, 93: 1607–1613, 1929.

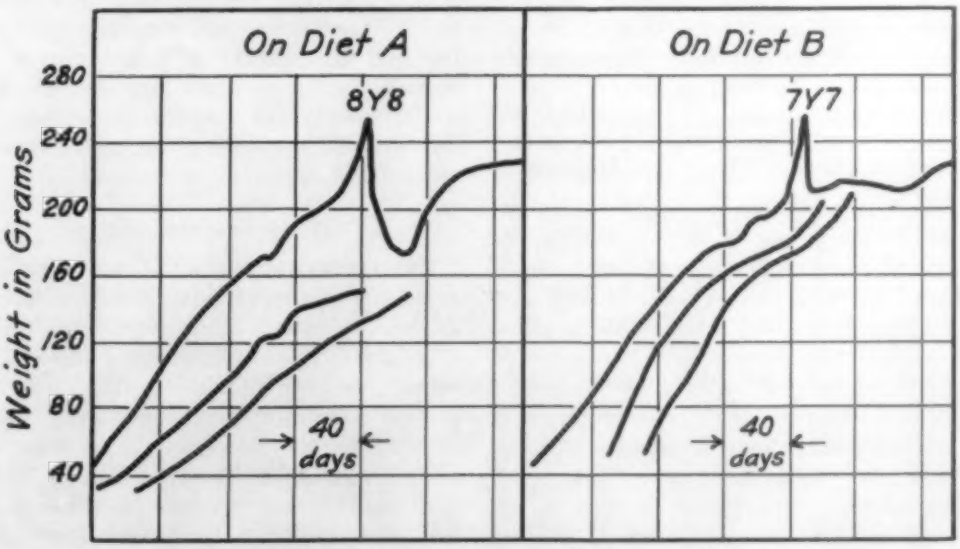

FIG. 4. WEIGHT CURVES.

Typical weight curves for a mother and two of her young on each of two diets. Both mothers reared all their first litters (8Y8 and 7Y7); but on Diet B the mother's body was better maintained, and also the young grew at a better rate.

promises, to those peoples who will use the newer knowledge of nutrition, greater vigor, increased longevity and a higher level of cultural attainment; and the representative of Australia commands the attention of the League of Nations with his slogan, "Marry Agriculture and Health."

Perhaps science will be joined by statesmanship in bringing these recent advances of knowledge into the service of human welfare. In any case, this newer knowledge of nutrition is widely taught in the schools, is at least beginning to be used in a growing proportion of homes; and will (whether rapidly or gradually) become more and more effective through an increasingly informed and intelligent consumer demand.

At the same time, further scientific research can be of great benefit in showing more precisely the possibilities in this field; and developing more fully the ways in which these possibilities can be brought into service with a minimum of encroachment upon, and also with a minimum of retardation by, social customs, individual preferences and economic problems. Many of the economic problems can, in fact, be helped to solution by this new scientific knowledge.

Attainment of "Buoyant Health"

And, so far as we can judge, the positive improvement of health, the induction of a more buoyant health, can be added to the gains in health, efficiency and longevity which are attained in other ways, as through genetics, sanitation of environment and training of mind and body. The concept of the nutritional improvement of such health as is already "passable" to a status more "buoyant" (to use the terms of the *Journal* of the American Medical Association) fits excellently the scheme recently offered here at the Institution from the genetic point of view (Fig. 6).

To picture, as an addition to the diagram in which the life cycle is represented by the path of a projectile, the improvement which can be made through nutrition, if we think in terms of *positive* health, is as if the projectile, instead of merely responding passively to its original impetus, were (like a rocket or torpedo) generating additional propulsive power during its flight; or, if we think in terms of the word *buoyant*, it is as if we had found a way of partly offsetting the downward pull of gravitation (of diminishing the rate at which we tend to age), which is essentially what the newer chemistry of nutrition is now doing in finding how to induce and maintain a *superior internal environment*.

Our actual experiments, thus far, are chemical and biological—not psychological—but so far as our minds are domiciled in our bodies it seems reasonable to expect that a superior internal environment may have a more than merely biological value.

At any rate, we may now accept as established, (1) the existence of a wide and evidently fruitful area of research between the merely adequate and the optimal in food supply and nutritional condition, and (2) the general validity of our present methods of animal experimentation in this field of research.

Further Investigations

The further experiments now in progress are designed to extend present knowledge in two main ways. We seek to establish the relationships of the individual chemical factors of food value to the nutritional improvements originally effected by adjustment of the relative proportions of ordinary articles of food. This should make possible such formulation as to facilitate the fullest functioning of these findings both in pure science and in medicine, dietetics, and food economics.

FIG. 5. AGING RATS.

THESE RATS HAVE ALWAYS BEEN HEALTHY AND NORMALLY NOURISHED, BUT WHEN PHOTOGRAPHED WERE BEGINNING TO SHOW AGING (COMPARE WITH THE HEALTHY YOUNG RAT IN FIG. 3). THE EQUAL DEGREE OF INCIPIENT SENILITY IN THE TWO CASES HERE SHOWN WAS REACHED, AS EXPLAINED IN THE TEXT, AT DIFFERENT AGES—ACCORDING AS THE DIETARY HAD BEEN MERELY ADEQUATE OR SOMEWHAT BETTER.

And, simultaneously, we seek also to ascertain just what kinds and degrees of enrichment of the dietary are effective for the nutritional improvement of health at *all* stages of the life cycle. Even during the time that our present (current) experiments have been in progress, this latter question has been given increased prominence by the tendency of certain pediatricians to warn against the assumption that the most rapid early growth is necessarily the most conducive to future well-being, and by the finding in the Agricultural Experiment Station at Cornell that a very rapid growth, induced by extremely rich diet given in unlimited amounts, may not always be best in its effects upon the life cycle as a whole.[5]

There is not necessarily any real conflict between the Cornell and the Columbia findings. If they appear divergent this may be simply because they deal with widely separate areas in what we now see to be a very broad field of investigation.

The starting point of the Cornell experiments was an extremely rich diet, such as may be approached in occasional cases of the forcing of farm animals for maximal gains in body weight; and possibly when an infant is fed without regard to economic considerations and with too great a desire to make a phenomenal record of growth at an early age. Such cases of undue forcing of growth by extreme richness of food, if they actually occur in practice, are doubtless relatively rare; they do not seem likely to affect more than an extremely small minority of the population.

The starting point of our experiments at Columbia was a dietary much more representative of the food supplies upon which the great majority of people must depend. The chief sources of food calories are the grain products, here represented by wheat, and the dietary is made adequate by the inclusion of an economical proportion of the so-called "protective" foods, here represented by milk.

FACTORS IN ENRICHMENT OF DIET

Between our Diets A and B the sole difference was the relative proportions in which these everyday foods were consumed. But the change in this single

[5] C. M. McCay, M. F. Crowell and L. A. Maynard, *Jour. Nutrition*, 10: 63–79, 1935.

experimental factor means, when the description of the experiment is transposed into chemical terms, the enrichment of the diet in not less than four respects: calcium, protein, and the vitamins A and G.

We seek now to establish, as rigorously and precisely as possible, the rôle in this connection of each of these four factors, beginning with calcium.

The minimal level of calcium intake for permanent support of normal nutrition appears, from the findings of several investigations, to lie between 0.13 and 0.19 per cent. of calcium in the dry food; while the optimal level is materially higher, though just how much higher is a problem still under active investigation.

Both by calcium-balance experiments with growing children[6] and by analyses of the bodies of experimental animals[7,8] it is found that even when there is every appearance of normal growth there may be considerable differences in the rate of calcium retention, depending upon the calcium content of the food.

Such data upon the period of growth and development are, of course, not conclusive as to the *ultimate* effects. But experiments recently completed, which cover the entire life cycle and extend into successive generations, indicate that the diets with moderately increased calcium content, which have expedited growth and development, with earlier attainment of an adult percentage of body calcium, have also induced higher vitality throughout and improved the life expectation of the adult as well as of the young.

[6] H. C. Sherman and E. Hawley, *Jour. Biol. Chem.*, 53: 375–399, 1922.

[7] H. C. Sherman and F. L. MacLeod, *Jour. Biol. Chem.*, 74: 429–459, 1925.

[8] H. C. Sherman and L. E. Booher, *Jour. Biol. Chem.*, 93: 93–103, 1931.

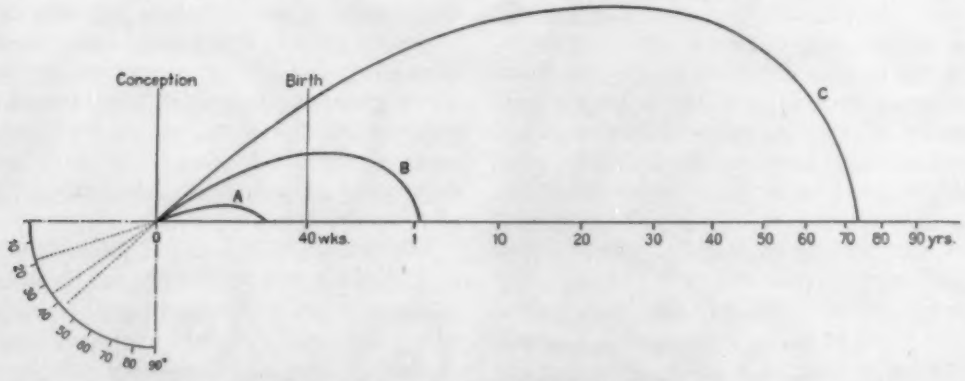

FIG. 6. LIFE AS A BALLISTIC CURVE.

LIFE IS SHOWN AS A BALLISTIC CURVE, THE FORM AND SPAN OF WHICH ARE DETERMINED BY: THE INITIAL QUALITY OF THE EGG-CELL AND THE RESISTANCE IT MEETS IN ITS FURTHER COURSE. IN THE THREE CURVES, CHOSEN AS TYPICAL, IT IS ASSUMED THAT THESE INDIVIDUALS ENCOUNTER THE COMMON WEAR AND TEAR OF EXISTENCE. THE DIFFERENCE IN THEIR LIFE-SPAN RESULTS ENTIRELY FROM THE DIFFERENCE IN THEIR INITIAL QUALITY. WERE THE WEAR AND TEAR GREATER OR LESS THE CURVES WOULD BE CORRESPONDINGLY ALTERED. IN SUCH A DIAGRAM THE RELATIVE INITIAL QUALITY OF THE EGG-CELL CAN BE EXPRESSED IN TERMS OF DEGREES OF THE ANGLE OF PRIMARY INCLINATION. THE NEWER KNOWLEDGE OF NUTRITION SHOWS HOW THE NORMAL PATH HERE PICTURED BECOMES BOTH HIGHER AND LONGER WHEN TO THE INITIAL IMPETUS THERE IS ADDED THE SUSTAINING EFFECT OF A BETTER-THAN-MERELY-ADEQUATE DIETARY WITH RESULTING NUTRITIONAL IMPROVEMENT OF THE INTERNAL ENVIRONMENT.

Effects of Increasing Calcium Content

With all other factors held uniform,[9] this moderate increase in the calcium content of the food has resulted in more rapid growth, more efficient utilization of food value whether computed in terms of calories or of protein, slightly earlier maturity, improvement of the already normal health at all ages, and some increase in the average length of adult life.

In this particular series, the gain in longevity by the males is undoubtedly significant, while that by the females is smaller and if it stood alone would not be statistically convincing. Before one concludes, however, that the nutritional reactions of the sexes are different in this respect, there should be similar experiments comparing the effects of the two food supplies upon the life histories of unmated females and upon groups of families in which the factor of pregnancy and lactation is held constant. For in the experiments just mentioned, the females on the food supply richer in calcium produced and suckled more young and it is conceivable that if the demands of pregnancy and lactation had been less frequent and heavy these females might have lived somewhat longer. It is hoped that opportunity can be found for such an additional series of experiments.

Experiments in Progress

Meanwhile, experiments are already in progress to determine the effects of further enrichment of the diet in its calcium and phosphorus contents,[10] and with and without simultaneous enrichment in protein content, vitamin values, or both.[11] Other work in our laboratory has suggested that both in the case of vitamin A[12] and of vitamin G[12] there is progressive nutritional benefit with increasing liberality of intake up to levels probably at least four-fold higher than the "actual" or minimal needs of normal nutrition.

We hope to continue the studies of the effects of these relatively high levels of intake of calcium, of protein and of vitamins A and G throughout the life cycles of at least two generations.

What level of consumption yields the best results is, therefore, a question whose answer, it now appears, will differ rather widely for different factors of food value.

Of food calories, the level of intake which yields the best results in the long run is very nearly the same as the minimum which will maintain a normal status.

Of protein, the optimum is now generally held to be not over twice the normal minimum.

Of calcium, the results thus far obtained seem to indicate that the best results are reached only at a relatively more liberal intake, perhaps about three times the normal minimum.

While with vitamins A and G, the present indications are that the best results may require at least four times as much as would cover the minimal normal needs.

The necessity for brevity prevents the discussion, here, of those aspects of our research which have shown that the growth data of our experiments have such a symmetrical frequency-distribution as to justify a high degree of confidence in their statistical interpretation[14]; and the further finding that faster or slower growth as an *individual* characteristic

[9] H. C. Sherman and H. L. Campbell, *Jour. Nutrition*, 10: 363–371, 1935.

[10] Unpublished experiments of E. W. Toepfer.

[11] Unpublished experiments of R. T. Conner and of L. N. Ellis.

[12] E. L. Batchelder, *Am. Jour. Physiol.*, 109: 430–435, 1934.

[13] H. C. Sherman and L. N. Ellis, *Jour. Biol. Chem.*, 104, 91–97, 1934.

[14] H. C. Sherman and H. L. Campbell, *Proc. Nat. Acad. Sci.*, 20: 413–416, 1934.

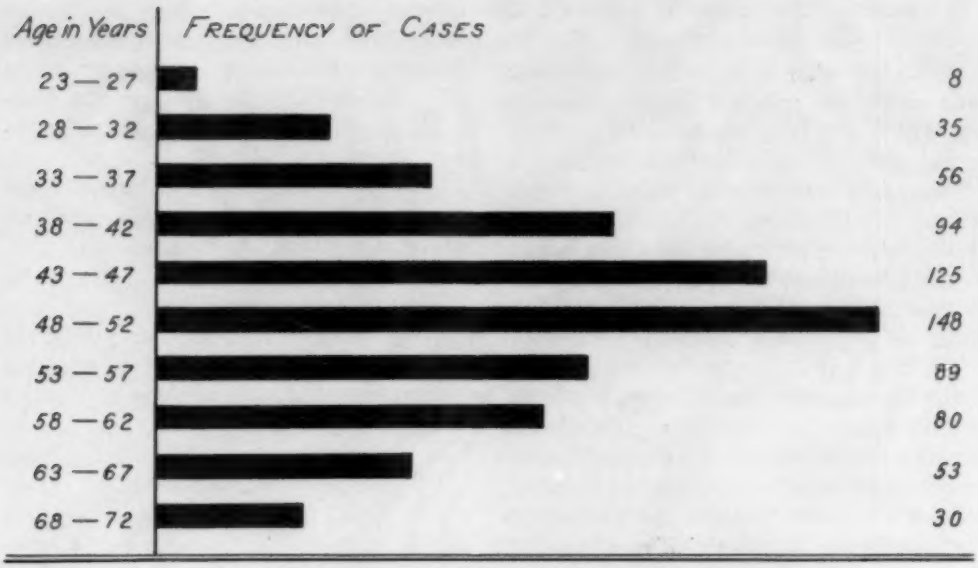

FIG. 7. AGE INCIDENCE OF MAJOR OPPORTUNITY.

FROM DATA DERIVED LARGELY FROM "AMERICAN MEN OF SCIENCE," FIFTH EDITION, THERE IS HERE CHARTED THE INCIDENCE OF MAJOR OPPORTUNITY IN DIFFERENT AGE RANGES, AS EXPLAINED IN THE ACCOMPANYING TEXT.

teristic does not in itself influence the life expectation. That is, the *strictly individual* variations in growth and in longevity are independent, not interdependent.[15]

These two findings place our research program in a much stronger position for the solution of the further problems mentioned above as still under investigation.

While the improvements in health and longevity, to which the newer chemistry of nutrition is showing the way, involve nothing so dramatic biologically as a mutation, or as some of the exploits of endocrinology, yet they seem to have a more than biological significance.

BROADER HUMAN IMPLICATIONS

May we conclude by glancing very briefly at some of their broader, and perhaps more far-reaching, human implications?

[15] H. C. Sherman and H. L. Campbell, *Proc. Nat. Acad. Sci.*, 21: 235–239, 1935.

In one of his annual reports, commenting upon the deaths of two colleagues, the late President Woodward remarked that a third of a professional or scientific man's years have usually passed by the time he has finished his formal schooling and entered his constructive life work; then probably another third will be spent in proving to himself and to others what he is able to do, before he will be entrusted with his highest responsibilities; and so, only the last third of his years remain in which to render his fullest service to the world.

A recent charting of the age incidence of major opportunities of presumably representative men in occupations of a scientific or related administrative or educational nature, strikingly confirms Woodward's impression that the most frequent time of attaining (or being promoted to) such "fullest" opportunity, is around the age of 50 years. Perhaps

equally striking is the wide range of ages at which appreciable numbers of men have actually found (or been admitted to) their major opportunities (Fig. 7).

Both these facts emphasize strongly the advantages to the individual and the gains to society which may confidently be anticipated from the earlier attainment and the longer retention of the full adult capacity and efficiency of individual persons (and of the constantly increasing proportion of people) who will have received the benefits of the newer chemistry of nutrition.

Such improvements should greatly facilitate what is now being found so important both in scientific research and in the industrial world, team work on terms of essential equality between younger and older people, to bring into the service of a given enterprise the full advantages both of the newer training and of the more mature experience, as well as of differing but mutually helpful points of view. And in proportion as work has social value, the value to society of longer individual careers (with full opportunity opened earlier in life and in ways which permit the full use of experience also) should be increasingly recognized.

As President Merriam has pointed out in his essay entitled "Are the Days of Creation Ended?",[16] the direction of human evolution is now largely social, and society is a continuing organism interested in its own future. What promises to affect this future should influence our decisions from day to day and will do so more effectively with the growth of the scientific spirit which expects progress and works for it; but meanwhile the shortness of individual lives tends to set a limit to the actual use by man of the knowledge which he has accumulated and the institutions which he has built and developed. Hence the longer term of fully efficient years which the newer chemistry of nutrition offers may be of far-reaching significance to human progress in affording fuller opportunity for the use and enjoyment of the ever growing body of knowledge.

[16] J. C. Merriam, "The Living Past" (Scribner's), 1931.

THE INFLUENCE OF SOLAR VARIABILITY ON WEATHER

By Dr. C. G. ABBOT

SECRETARY, SMITHSONIAN INSTITUTION

METEOROLOGISTS with few exceptions have attributed weather changes to the complex make-up of the earth and its atmosphere. Certain observations and statistical studies lead me to believe that an important factor in weather production is the fluctuation of the intensity of the sun's radiation. Hitherto no attention has been given to this factor in official forecasting. The following paper indicates the magnitudes of weather changes which solar variability may produce. Whether or not these effects may become readily predictable, their recognition seems to me to be a step in advance.

Viewing the sun as a variable star, its percentage range of variation is not large. Careful measurements made by the Smithsonian Institution at favorable mountain stations in desert lands since 1918 do not disclose a total probable range in the solar constant of radiation of more than 3 per cent. It is quite possible, but not capable of proof as yet, that larger solar changes have occurred in the past and may occur in the future. But this is outside of our present inquiry.

Fig. 1 shows a series of solar constant observations made on every suitable day from June 3 to November 2, 1934, at two stations. Mount Montezuma lies in the southern hemisphere, Lat. 22° 40′ S., Long. 68° 56′ W.; Alt., 9,000 feet. Mount St. Katherine lies near Mount Sinai in Egypt, Lat. 28° 27′ N., Long. 34° 0′ E.; Alt., 8,500 feet. Thus the local conditions which might have affected these results included summer at St. Katherine and winter at Montezuma. Yet the average deviation of the daily values, including all which are graded by the observers as satisfactory, S, or nearly satisfactory, S–, is 0.4 per cent. This means that the methods and observations are so satisfactory that results of two stations 7,000 miles apart, one operating in winter, the other in summer, are little affected by instrumental errors and by the variable conditions prevailing in the ocean of atmosphere above them. For assuming that the two stations are equally good, we find their individual average daily probable error in estimating the intensity of solar radiation in free space at the earth's mean solar distance is only $\frac{0.4 \times 0.8}{\sqrt{2}}$ or 0.25 per cent.

To the eye, Fig. 1 seems to indicate several variations of the sun of the order of 1 per cent. But when we compute correlation coefficients from all these data of the two stations which are marked S or S–, the value comes out insignificant. Everybody knows, of course, that such is always the case when a small real variation is associated with relatively large values of probable error. But in this case one other justifiable step may be taken. Certain values may be omitted, not because they spoil the correlation coefficient, but because they lie wild in the series observed at their own station. For instance, consider the following case. The figures given below are in thousandths of a calorie. For completeness read 1.900 plus the values given.

ST. KATHERINE

July	21	22	23	24	25	26	27	28	29	30
Values	46	43	37	39	45	32	45	44	48	47
Grades	S	S	S–	S	S	S–	S–	S–	S–	S–

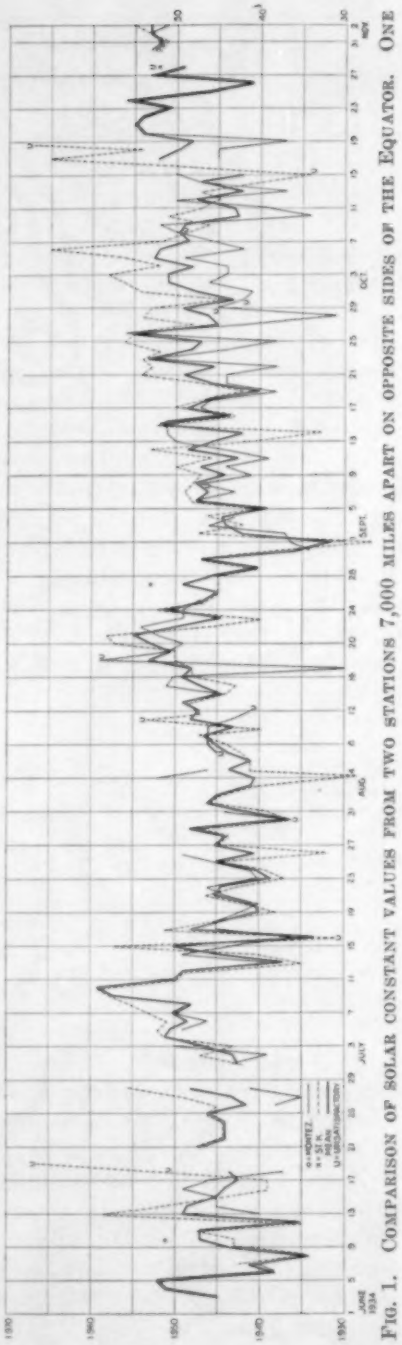

FIG. 1. COMPARISON OF SOLAR CONSTANT VALUES FROM TWO STATIONS 7,000 MILES APART ON OPPOSITE SIDES OF THE EQUATOR. ONE PER CENT. EQUALS FOUR VERTICAL DIVISIONS.

It seems very clear that though to the observers there was little visible objection to the value on July 26, yet it lies so wild compared to its neighboring days that to include it in a correlation computation relating to a possible variation of 0.020 calorie in range it must greatly reduce the resulting correlation coefficient. Acting on this line, Fig. 2 represents a spot diagram in which obviously unsatisfactory days have been eliminated in this way, notwithstanding their fair grades as rated by the observers. All the days so eliminated are tabulated in Fig. 2, and the reader may compare them with Fig. 1. As amended, the data show an obvious correlation, confirming our impression that variations of the sun occurred between June 3 and November 2, 1934.

Still the reader may object to what has just been suggested. I therefore undertake another line of argument. Let us assume that the sun really varies at short intervals, as Fig. 1 indicates. Further, let us assume that such solar variations produce sensible weather changes, in temperature, for instance, at Washington. It would naturally follow that whatever the effect might be when the solar constant increases for several days, the opposite temperature effect would follow when the solar constant declines.

With this idea in mind I have sought out *all* the cases occurring between January, 1924, and December, 1930, when sequence of several days indicated rise or fall of solar radiation. The values of the solar constant discussed are those observed at Montezuma, as printed in *Annals, Astrophysical Observatory,* Vol. 5, Table 31. I did not include sporadic single-day jumps but only those occasions when several days indicated a real rising or falling sequence. Nevertheless,

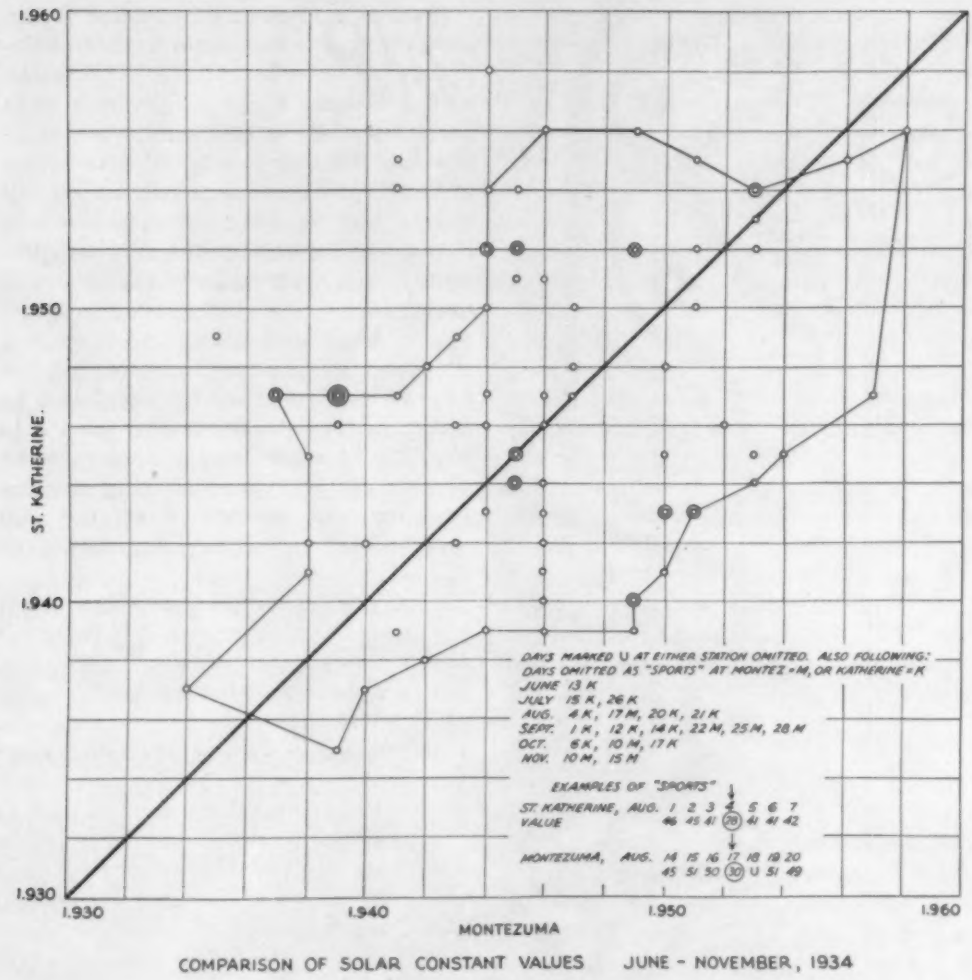
1.960
1.950
1.940
1.930
ST. KATHERINE.
1.930
1.940
1.950
1.960
MONTEZUMA
DAYS MARKED U AT EITHER STATION OMITTED. ALSO FOLLOWING:
DAYS OMITTED AS "SPORTS" AT MONTEZ = M, OR KATHERINE = K
JUNE 13 K
JULY 15 K, 26 K
AUG. 4 K, 17 M, 20 K, 21 K
SEPT. 1 K, 12 K, 14 K, 22 M, 25 M, 28 M
OCT. 6 K, 10 M, 17 K
NOV. 10 M, 15 M
EXAMPLES OF "SPORTS"
ST. KATHERINE, AUG. 1 2 3 4 5 6 7
VALUE 46 45 41 (28) 41 41 42
MONTEZUMA, AUG. 14 15 16 17 18 19 20
45 51 50 (30) U 51 49
COMPARISON OF SOLAR CONSTANT VALUES JUNE - NOVEMBER, 1934

FIG. 2.

it is highly probable that some of my cases are spurious. Hence the temperature effect caused, if real, is the more unlikely to be found. None of the changes of solar radiation were on their face values large. Their average could not have exceeded one half per cent.

The temperature departures used are the differences between the reported daily means of Washington maximum and minimum and a smooth curve drawn to represent the monthly means of this quantity as published in the first volume of *World Weather Records*. It is not to be supposed that the temperature effects, if real, caused by solar changes would be the same at different seasons of the year. Hence I segregated the results by months, throwing, for example, all cases in which the solar constant showed rising sequences in the months of January between the years 1924 and 1931, inclusive, into one table, and so on.

Fig. 3 shows the actual result of this experiment for the month of April, which is one of the better months, but

in no way exceptional. The reader will note the marked opposition of temperature departures at Washington, covering even details following, respectively, rising and falling sequences of solar variation. The spread of temperatures is very large, amounting, respectively, to 12° and 11° F., two days and twelve days after the solar change began which seems to have caused it. All other months show similar oppositions of effect, and many months show as large effects as April.

The actual march of temperature departures in Washington following solar changes alters from month to month. Rising tendencies follow rising solar sequences for the months December to April, inclusive. The opposite is the rule from May to August, inclusive. The other months show well-marked opposition, but in irregular types of march. It is, of course, probable that some other classification than a monthly one would be preferable. The type of temperature march doubtless depends on the pattern of the atmospheric circulation by which weather influences of solar origin arrive at Washington. For they immediately affect certain centers of action thousands of miles away, and travel through the atmosphere by roundabout paths. Further study of the phenomena may lead to a better grouping than the monthly one.

Much further investigation of the dependence of temperatures on short-interval solar changes is contemplated. Here it needs only to be added that the later solar constant work since 1930 leads to demonstrations of opposition of temperature effects similar to that of 1924 to 1930. What is certainly extraordinary is the apparent fact that one half per cent. change in the solar constant of radiation may produce 5° to 7° F. change in temperature departures, and

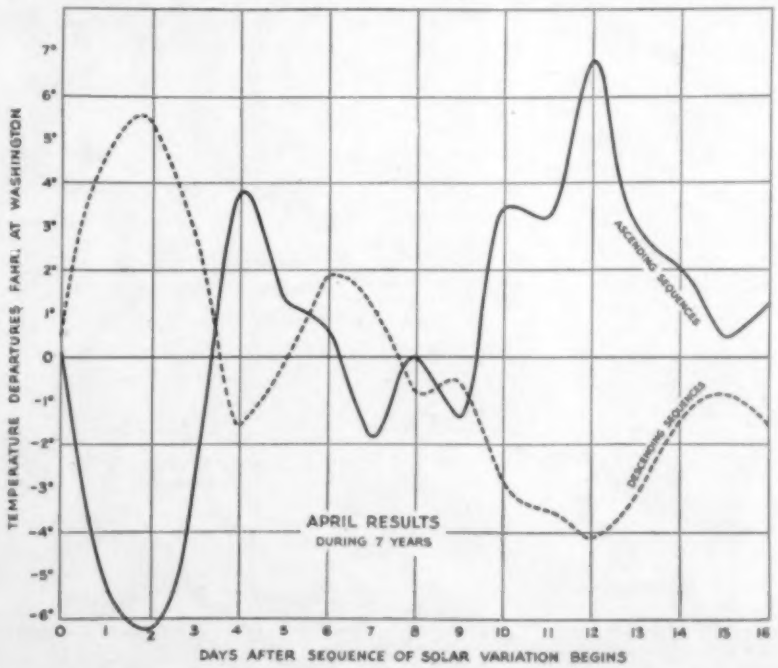

FIG. 3. OPPOSED TEMPERATURE DEPARTURES WHICH FOLLOW RISING AND FALLING SEQUENCES OF SOLAR VARIATION.

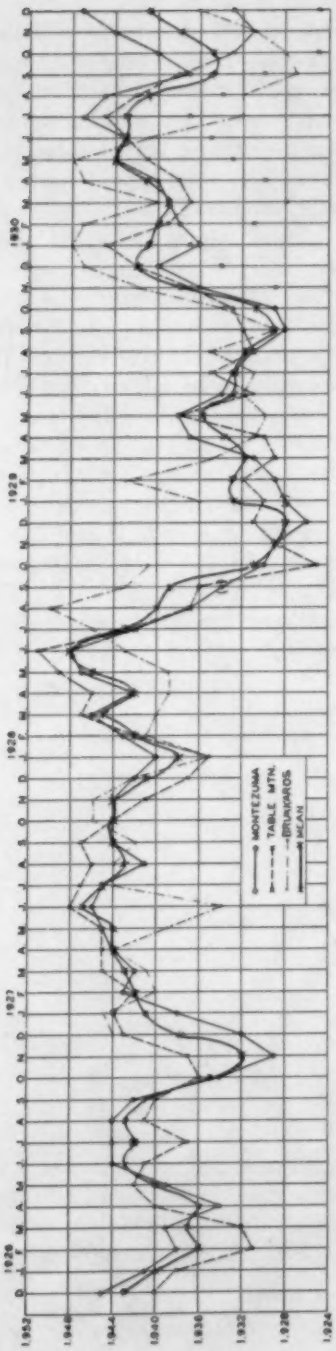

FIG. 4. COMPARATIVE RESULTS OF SOLAR CONSTANT OBSERVING AT THREE WIDELY SEPARATED STATIONS.

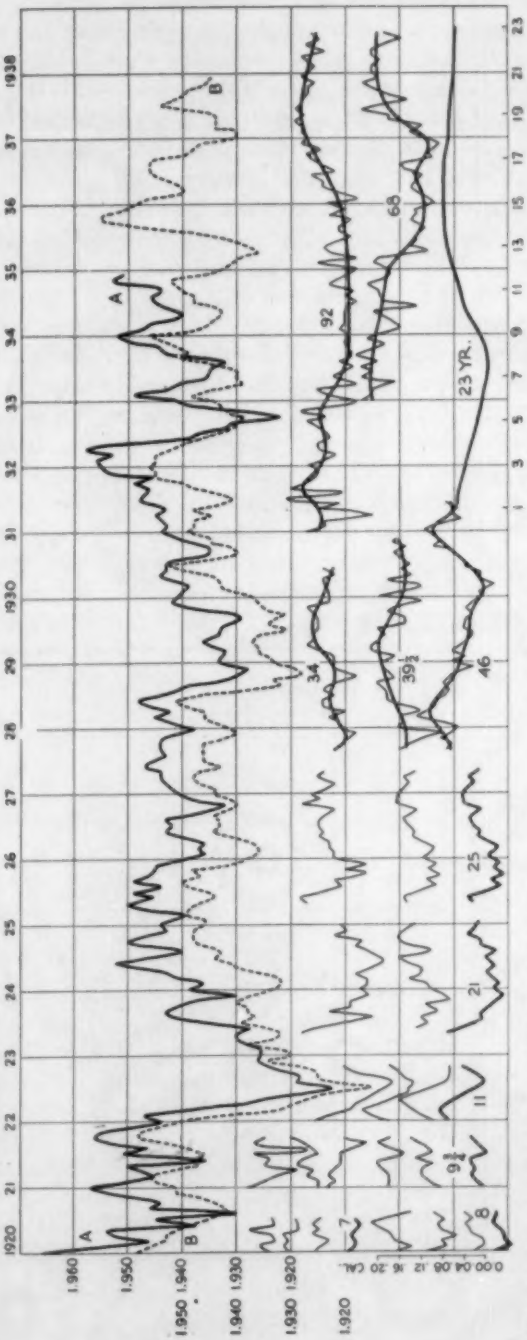

FIG. 5. SOLAR VARIATION, 1920 TO 1934, WITH PERIODICITIES FOUND THEREIN AND THE APPROXIMATE REPRODUCTION OF THE ORIGINAL CURVE BY THEIR SUMMATION.

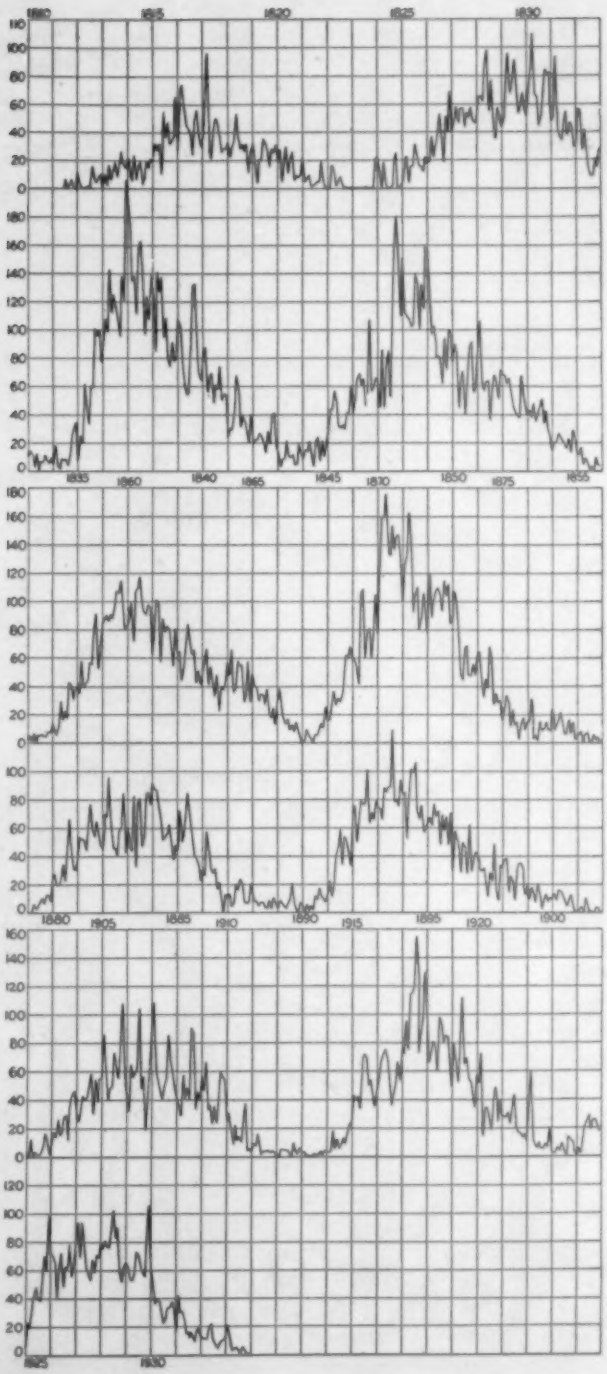

FIG. 6. PERIODICITY OF SUN-SPOTS SINCE 1811. NOTE THE LEFT CURVES ALWAYS SMALLER THAN THE RIGHT, INDICATING A 23-YEAR PERIOD.

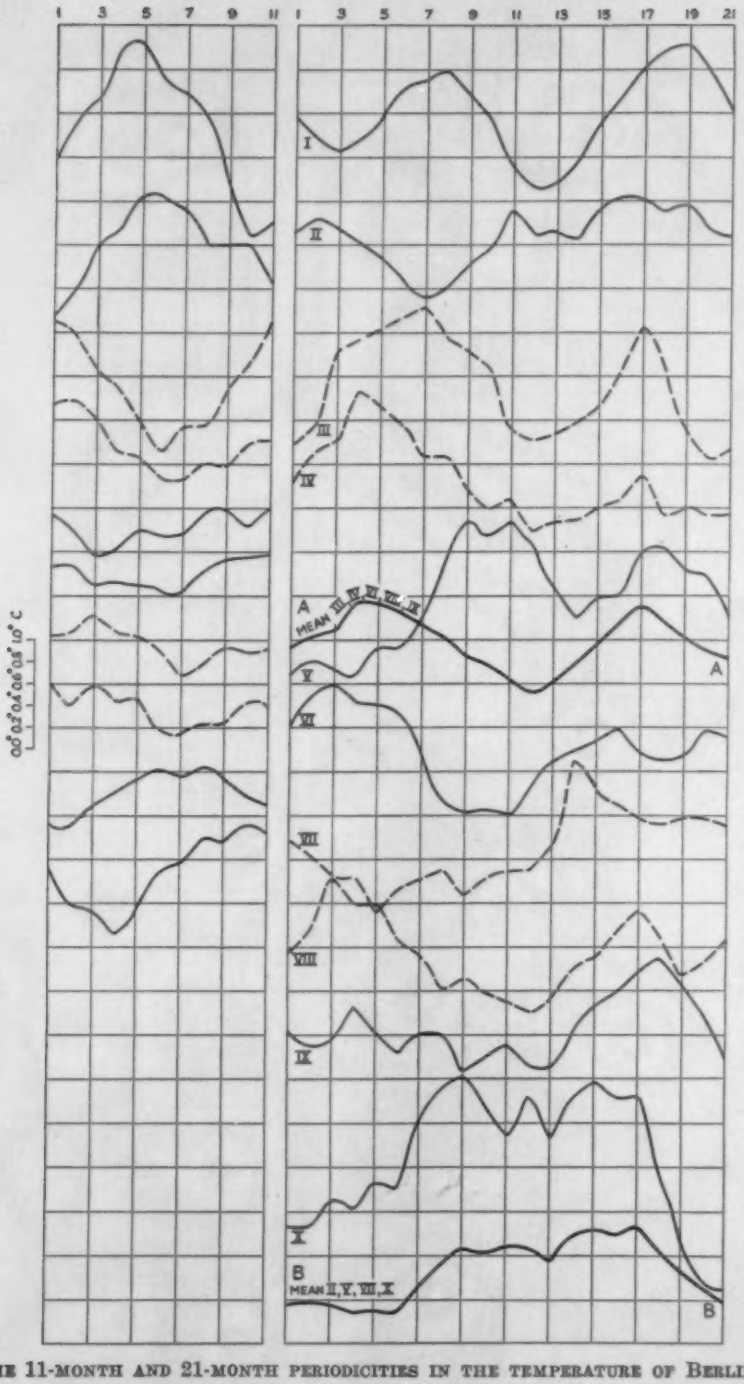

FIG. 7. THE 11-MONTH AND 21-MONTH PERIODICITIES IN THE TEMPERATURE OF BERLIN. NOTE REVERSALS AT MULTIPLES OF 11½ OR OF 23 YEARS FROM 1819.

may extend its influence over a period of two weeks after the solar change begins. I have as yet made no attempts to use this relationship for forecasting.[1]

We now turn to longer intervals. In Fig. 4 is shown the mean monthly march of solar constant values as observed from 1924 to 1930 at Montezuma, Table Mountain and Mount Brukkaros, stations separated by many thousand miles, and in opposite hemispheres. The reader will perceive that though divergences occur, on the whole the three stations afford mutual support. When we reflect upon the small probable error of an individual day's work at such a station as Montezuma, as stated above, and that it is to be divided by $\sqrt{16}$ or thereabouts in a monthly mean, one sees no reasonable doubt that the fluctuations are large enough to prove that the sun did actually vary as shown. Fig. 5, Curve A, gives the best result from 1920 to 1934.

Analysis of the solar variation indicated by Fig. 5 discloses that it may be represented, to an average deviation of one fifth of one per cent., as the summation of the following twelve periodicities: 7, 8, 9¾, 11, 21, 25, 34, 39½, 46, 68, 92 and 276 months. For the shorter of these periods the data may be separated into two or more parts, so as to see if the period holds in form and phase throughout. In Fig. 5, I show such an analysis of the data. The heavy lines give the average form and phase of the twelve periods. Their summation in the dotted curve, B, may be compared with the original data in Curve A. The most interesting thing about this analysis is that it was only after about half of the periodicities had been discovered independently, one by one, that it was noticed that they have an approximate least common multiple in 276 months, or 23 years.

[1] A completer study of these phenomena is about to be published by the Smithsonian Institution.

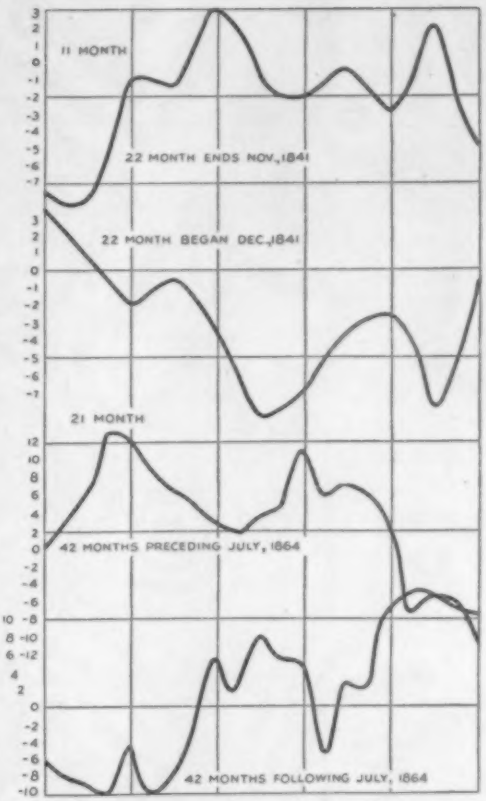
11 MONTH
22 MONTH ENDS NOV., 1841
22 MONTH BEGAN DEC., 1841
21 MONTH
42 MONTHS PRECEDING JULY, 1864
42 MONTHS FOLLOWING JULY, 1864

FIG. 8. SHARPNESS OF REVERSALS IN BERLIN TEMPERATURE PERIODICITIES.

Then it was recalled that Hale had discovered that the magnetism of sun-spots shows a complete cycle composed of two 11-year sun-spot periods. Still later I have noticed that the sun-spot cycles themselves have an alternate inequality, and therefore a complete period of two 11-year cycles. This is seen clearly in Fig. 6, where every left-hand curve includes a less area than its right-hand neighbor.

I do not pretend to explain why the sun's radiation displays as periodicities a fundamental and eleven overtones. In the vibrations of a violin string this would not be remarkable, but why it should occur with a sphere of perfect gas I must leave to mathematicians.

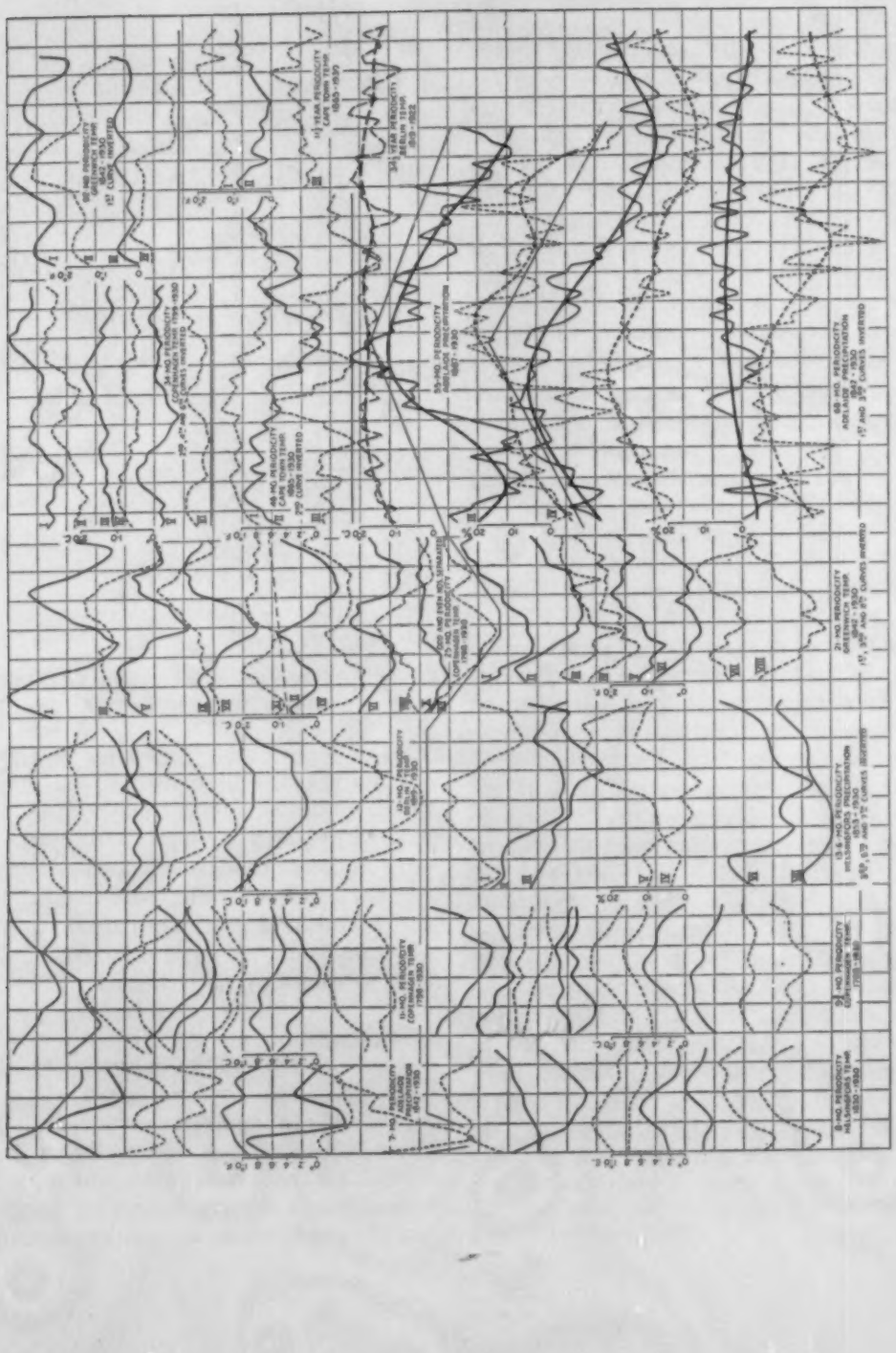

FIG. 9. SAMPLES OF PERIODICITIES IN WEATHER FROM VARIOUS STATIONS. NOTE CONTROL BY 23-YEAR CYCLE.

Similarly, I do not pretend to explain why as indicated in Fig. 3 a sequence of solar change ranging only about one half per cent. should cause a departure from normal temperatures at Washington of 5° to 7° F. I am presenting statistical results in this paper, not theories to account for them. But I should be immensely pleased and relieved if some one of far abler mathematical powers than mine would explain them.

Assuming these twelve long solar periodicities shown in Fig. 5, what if any evidences are there that they affect weather? In this inquiry I have used the data printed in *World Weather Records,* published by the Smithsonian Institution. As monthly values of weather departures from normal fluctuate widely, I have smoothed such departures by consecutive 5-month means. In the use of precipitation, I have expressed it in percentages of normal, thus avoiding the embarrassment of rainy and dry seasons of each year which widely modify absolute values.

Having, then, such smoothed departures from normal, I tabulated them in ways adapted to disclose the periodicities discovered in solar variation. An illustration of such a tabulation may be found by interested readers on page 37 of my former paper.[2]

I found after much tabulation that a very significant relationship obtains. If one takes January, 1819, as the zero date, he finds that the reversals of phase, which hitherto have baffled seekers for weather periodicities, take place sharply at dates separated from January, 1819, by multiples of 11½ years.

Figs. 7 and 8 illustrate this discovery. Fig. 7 shows the 11-month and 21-month periodicities in the temperature departures of Berlin. It will be immediately perceived that the 11-month curves

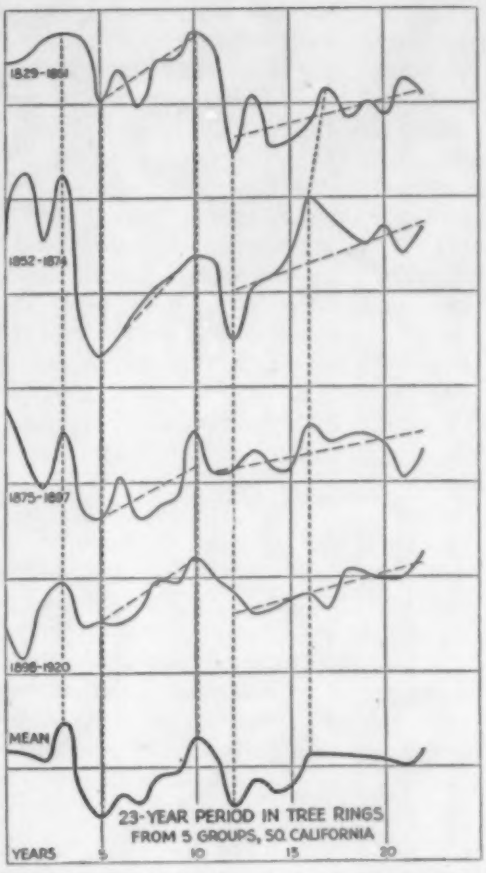

FIG. 10. THE 23-YEAR CYCLE IN DOUGLASS'S TREE-RING WIDTHS.

go in pairs, with a marked change of form occurring between each pair and the next. The individual curves of each pair represent 11-year and 12-year averages, respectively. Thus every pair covers 23 years. As for the 21-month curves at the right, they also cover alternately 11 years and 12 years, but their changes of form take place sometimes after 11½ years, sometimes after 23 years, but always at dates related to January, 1819, by intervals which are multiples of 11½ years.[3]

[2] *Smithsonian Misc. Coll.,* Vol. 94, No. 10, 1935.

[3] In Fig. 7, A, for "Mean III, IV, VI, VIII, IX" read "Mean I, III, IV, VI, VIII, IX."

Fig. 8 shows how very sharply this relationship with January, 1819, holds. The upper pair of curves represent, respectively, the 11-month period computed from the last 22 months before November, 1841, and from the first 22 months after that date. The lower curve represents the 21-month period computed from 42 months preceding and following, respectively, July, 1864. Both pairs show opposition even in details, and indicate complete reversal of phase at dates sharply 23 and 46 years after 1819.

I submit that the reversals of phase, which at first sight obscure solar periodicities in weather, turn out to be of first-rate evidential quality, confirming the importance of solar variation as a weather factor. It will be clear, however, that, hampered by these reversals of phase, it becomes impossible to determine with satisfactory neatness the forms of the curves representing the effects of the longer solar periodicities on weather phenomena. For however long a weather

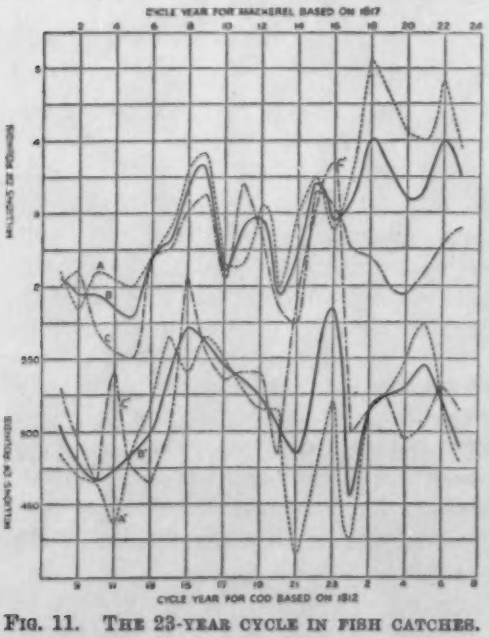

FIG. 11. THE 23-YEAR CYCLE IN FISH CATCHES.

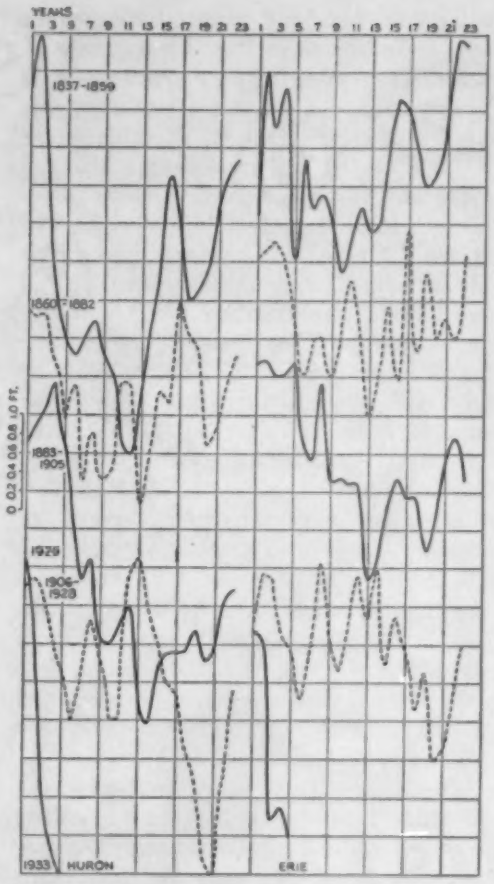

FIG. 12. LEVELS OF GREAT LAKES, 23-YEAR CYCLES. NOTE THE MARKED SUBSIDENCE CULMINATING AFTER 11 YEARS IN THE FULL CURVES, INDICATING DOUBLE CYCLE OF 46 YEARS GOVERNING DROUGHT CONDITIONS.

record is available, it is broken up, as the 11-month and 21-month curves of Fig. 7 show, into successive intervals when the solar influence combined with terrestrial complexity produced different forms of curves representing weather periodicities. This is not fatal to getting good results for shorter periodicities, but leaves us very much embarrassed as regards the longer ones. We lack sufficient repetitions to eliminate accidental and short periodical features from them.

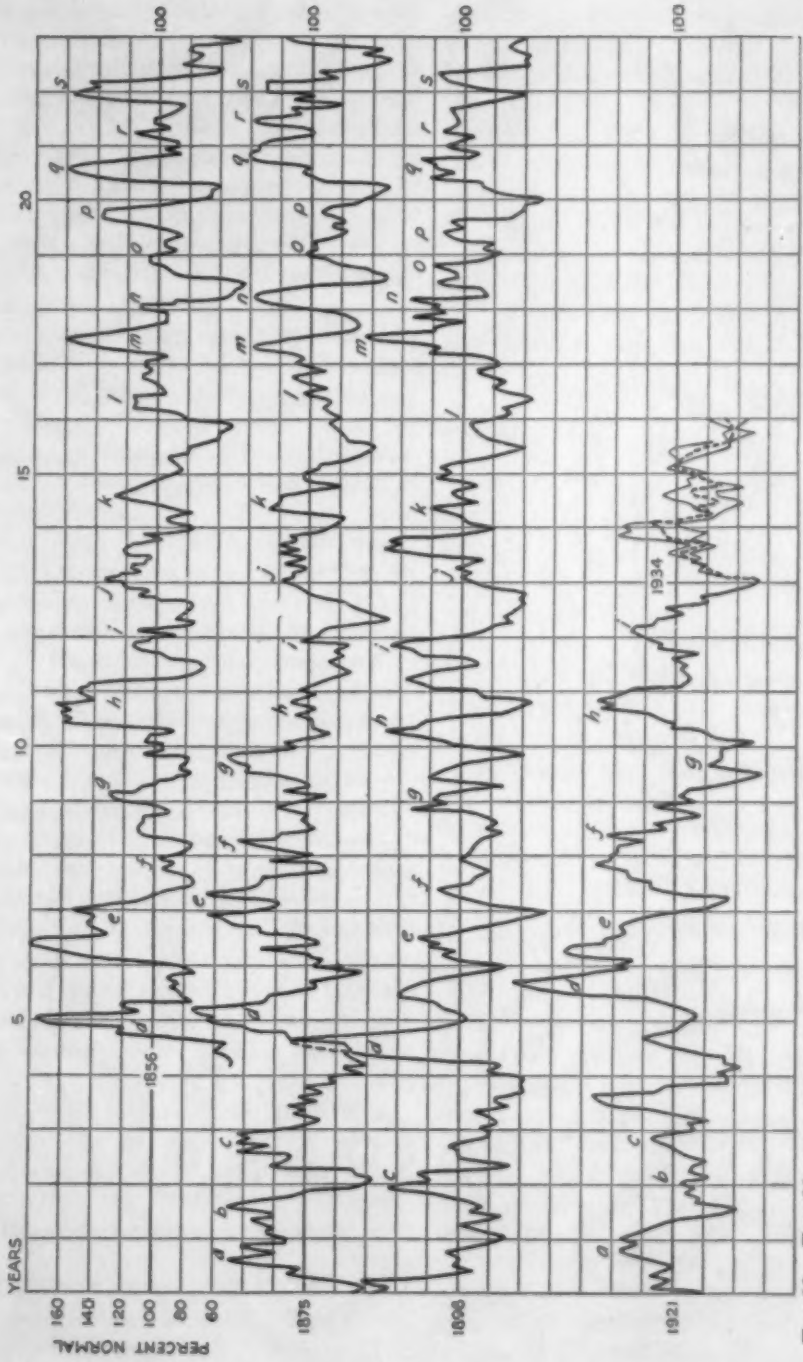

FIG. 13. THE 23-YEAR CYCLE IN THE PRECIPITATION OF PEORIA, ILL. YEARS 1934, 1935 AND 1936 PREDICTED FROM PREVIOUS DATA, AND EXPRESSED BY THE DOTTED CURVE. CORRESPONDING FEATURES IN THE SEVERAL CURVES ARE MARKED BY CORRESPONDING LETTERS.

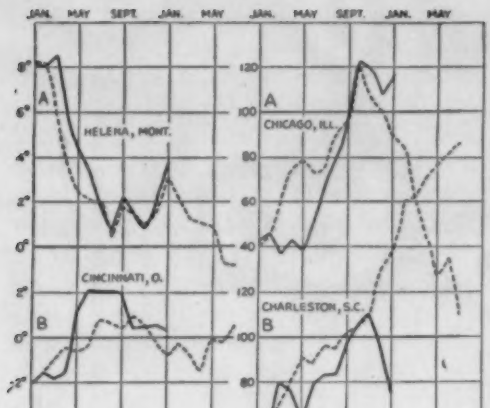

FIG. 14. SAMPLE FORECASTS AND VERIFICATIONS. DOTTED CURVES ARE FORECASTS. GRADES OF RESULTS: A, EXCELLENT; B, FAIR; C, HALF AND HALF; D, BAD. LEFT, TEMPERATURE; RIGHT, PRECIPITATION.

With these introductory remarks I refer to Fig. 9, which shows samples of all weather periodicities found, but scattered to represent six stations in different parts of the world, sometimes relating to temperature and sometimes to precipitation in the departures from normal weather investigated. Scales of ordinates are annexed for each group of curves. For the shorter periods, two determinations for each 23 years are given, but for periodicities of 34 months and over, only one. Successive 23-year intervals are distinguished by using alternately full and dotted lines. In order to save space, the successive curves representing the longer periodicities are drawn closer together by inverting those so designated in the legends. There are so many curves shown in Fig. 9 that they may prove bewildering. But I believe the profound changes of form, even amounting to inversions, at intervals of 23 years, are so marked that the reader will not overlook them. See, for instance, the 11-month periodicity of Copenhagen temperatures, the 68-month periodicity in Adelaide precipitation and, strangely enough, as many readers may think, the 12-month periodicity in Berlin temperatures. Everybody would be prepared to find a 12-month periodicity, but who would anticipate that its form would change regularly at 23-year intervals?

We now turn from this branch of the investigation to see how plainly the 23-year cycle and the double cycle of 46 years show themselves in some phenomena which depend on weather. In view of what has been explained we are not to expect sweeping curves of 23-years' interval. Rather, as the subordinate periodicities which the sun's radiation contains impress themselves on weather, numerous features will occur, as the crests and troughs of the periodic solar influences reinforce one another. Owing, however, to the irregularity of behavior of the different periodicities as regards reversal and gross changes of form (see Figs. 7 and 8), the features will sometimes be altered or obliterated or changed somewhat in phase.

Fig. 10 is obtained from data on the widths of tree rings in California, as measured by Douglass in five groups covering the years 1829 to 1920. Many details are seen to repeat themselves in all cycles.

Fig. 11 is a comparison of the 23-year cycles in the catch of cod and mackerel, 1812 to 1931. The values are averaged in alternate 23-year cycles, so as to bring

out the 46-year double cycle, if present. But it is not very conspicuous. I would therefore draw attention only to the heavy mean lines, which seem to show that, either directly or through altering the plankton food supply, these two species of fish go through somewhat similar cycles of abundance. The phases occur 2 years later for the cod than for the mackerel. For details of this fish study the reader is referred to the publication above cited.

Fig. 12 shows how marked is the 46-year double cycle in the levels of Lakes Huron and Erie. It is clear that the dry years since 1930 in the Northwest had their prototypes 46 and 92 years earlier.

Fig. 13 shows the 5-year consecutively smoothed precipitation of Peoria, Illinois, arranged in cycles of 23 years. Certain features are indicated by letters above the several curves. As remarked above, these features change somewhat in form and phase from cycle to cycle, and yet, I think, remain recognizable. Based on this belief, I ventured to produce on the lowest curve a continuation, which is, in effect, a forecast for 1934, 1935 and 1936. The year 1934 having elapsed, I have compared the event with the prediction, and when April, 1936, comes, so that 5-month consecutive means may be computed, I will do so for 1935.[4] Of 1934, I may merely remark that the prediction for Peoria precipitation falls in the class called "excellent" in the succeeding paragraph, as illustrated for Chicago in Fig. 14.

In Fig. 14, I show four types of agreement which represent the outcome of comparisons of forecasts for 1934 with the events. Curves on the left are for temperatures, and those on the right are for precipitations. The numbers of stations where predictions fell in the several classes are as follows:

Type	Excellent	Fair	Half good	Bad	Total
Temperature	7	17	3	4	31
Precipitation ...	11	11	8	5	35
Total numbers ..	18	28	11	9	66
Per cent. of total	27	42	17	14	100

The experiment is of course so new and startling that I have not felt warranted in disclosing the individual forecasts until several years of testing shall have elapsed. As stated above, the irregular reversals and changes of form in the constituent periodicities must ever render such forecasts based on the 23-year cycle subject to error in phase and amplitude. These may indeed prove to detract ruinously from their value unless it may prove possible to forecast reversals and form changes of all constituent periodicities. But of the thesis that the sun's variation is an important weather factor, hitherto generally unrecognized, I, for one, have no doubt.

[4] This comparison has been made. It is fairly successful for precipitation, but less so for temperature.

THE SO-CALLED SCIENTIFIC METHOD AND ITS RÔLE AS A PROCESS IN DEMOCRACY

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In respect to other things there may be times of darkness and times of light; there may be risings, decadences, and revivals. In science there is only progress. . . . What is gained by scientific inquiry is gained forever; it may be added to, it may seem to be covered up, but it can never be taken away.—*Sir Michael Foster.*

THERE have been rather numerous indications in recent years that the contributions of science, or better, of the scientific method, to modern life are not appreciated in many quarters. In this essay it will be shown that the method of science is not a modernized version of the mystical black arts of other times or places, but that it is actually a very ancient and socially secure method of achieving progress that has rather recently been taken over, perfected and named by science.

Not only have the contributions of science not been appreciated, but some have said that the dilemma in which the world finds itself to-day should frankly be blamed on science. These would have us believe that science has destroyed our religion, and therefore our ethics, by casting doubt on the literal accuracy of the story of creation as given in Genesis, and all that is linked with the idea of the literal accuracy of all statements in the Bible and by conquering our material and energy environment so far as to greatly displace human labor with machines and thereby creating technological unemployment. These charges would be serious if true.

But the bad effects of science, we are told, do not stop here. Not only do they say that it robs man of his old-time religion and honest employment for ten to fourteen hours a day, but they also say that it leads him to see himself as a helpless unit in a social order that deals with him in a statistical manner. So many of him are to be rich, so many comfortable, but too many shall perish, and this holds for spiritual as well as intellectual and material riches. All this is said to be controlled by the blind forces of inevitable laws of nature. The angry but approachable God of old, who punished his sinful subjects, is replaced by the impersonal operations of a cold statistical law. Moreover, the blind forces of the society in which we live are said to be the outcome or summation of other blind forces that control our own being. Thus, the will of man is not free, but is under the control of blind neurological and hormonal forces, or drives. How can a man behave himself when his hormones are tormenting him? Can he by taking thought change the quantity of these? Are they not in turn actuated by more primitive but even more blind drives? Finally, we come to the scale of molecules and atoms and these in turn are helpless in the blind fields of energy that fill the universe. It is the helplessness of these that is built up into a helplessness of the nth degree in society as a whole. As long as man was ignorant of the kind of a world he lives in, life was tolerable, because self-deceptions could be sustained, but now that the false façade has been removed by science, nothing of any beauty or value remains.

At our first sight of this hell on earth, we rebel in disbelief or else yield and flow along with the tide. But we shortly discover that neither response is satisfying. The disbeliever, who is attempting

to swim against the current, controls his desperate efforts and floats from time to time, and discovers that there seems to be direction to his floating movement. The yielder finds himself attempting to stem the tide at times because he appears to be getting nowhere by passive acceptance. It seems that we are so constituted as neither to be able to yield passively to the universe in which we live nor to stem its relentless flow. But even in this alternation of states we are not free. In the evening of the day or of life, the tendency to float is greater. In the morning of the day or of life, the tendency to swim against the current is greater. So here again a closer view shows this alternation of states itself to be under the influence of another set of inevitable drives.

For those who dwell on a picture of life as thus briefly and incompletely sketched, the whole idea that man can permanently emerge from his present state through any efforts of his own is considered to be highly improbable, if not impossible. Statistically, it is provided that a few individuals shall emerge, but only temporarily. They can not carry the mass along. They are said to be like bright meteors in the dark night, and the darkness following their extinction is worse than it was before. These few can control their spirits, or love their neighbors, or control their lusts, or see a higher destiny for man, but only for a few years or a life-time. After that things fall back to the dead level again. To believe differently, they would say, is to be deluded, to forget the rational implications of man's origin, his nature and his existence. Moreover, they remind us, man's powers at their best are still finite in an infinite universe, and the duration of his life but the tick of a watch in an eternity of time.

All the above and a great deal more is not to be directly or completely disputed, but is it not possible that the picture suggested is a partial or foreshortened one?

However, if we are to dispute implications of the sort considered above, we must broaden our approach. For some of us this is easy. For these the answer is intuitively given, and in some important respects ultra-rational or supra-rational. A few, whose spirits respond fully and clearly to great music or poetry, may even smile at the necessity of making a laborious answer, or even at giving serious consideration to such a view-point. To these we freely admit that our rationalistic and materialistic preoccupations may have changed us, but we must also insist that these preoccupations were our task. These achievements in science, that now appear in a disturbing form, have come from the execution of what seemed to be tasks assigned to our time and to our generation. It seems to many of us that we were obliged to do these things; they were our calling in life. But now in the cool of the evening after a period of achievement, we must be permitted to see what we can say to clarify this partial or foreshortened view of the fruits of our labors. As yet we can not see ourselves as Samsons, who have been pulling down the pillars of our own world, and in doing so, inadvertently accomplishing our own suicide.

In this case the historical view is the one that we can best adopt. Science as we know it to-day is the ripening fruit of a long period of growth and preparation. The pessimistic view that we have attempted to represent would lead us to believe that man himself can not achieve permanent progress. Whatever basis for this view may exist in some directions, it is not true in the field of medicine, for instance. Here we surely have obtained progress and improvement. The life of man has been bettered, at least so far as its comforts and duration are concerned.

This has come from stemming the tide, from opposing the flow of the infinite and inevitable forces of destruction inherent in man's own constitution and in the world around him.

My disconsolate friend may object and say that this, too, is under the control of immutable law. We shall not argue the point, because we, too, believe that it is. However, the law that we have in mind is different in that it permits of change. This law works in such a way that if one generation of men can contrive to pass on to another generation the arts that it has inherited and invented, that generation can use these heritages as a basis or springboard for new developments. This law is also unique in another respect, and that is that man seems to be the only living creature that has become the beneficiary of it, within the range of consciousness. Most of the processes taking place in a man, below the level of consciousness, are a heritage from ancestors, passed on to him by a process of inheritance below the level of consciousness. But these arts that we speak of are carried on in consciousness. Man may accept and pursue them or refrain from doing so, at a price. Man is thus seen to be subject to two kinds of inheritance: the one kind, that he has in common with all other creatures, is, comparatively speaking, not under conscious control, and is the more primitive of the two; the other is in some respects under conscious control, and in this man has more freedom. By this I mean that a man in mating can not say just which of the qualities that he and his mate can transmit shall appear in the child that is to be born, but that the child that is born can in some important degree determine whether a strong linguistic or mathematical bent shall be expressed and developed, if at all, in one way or another.

Considered from this standpoint, the whole known history of man is the record of his attitude toward this law. The oldest known culture of the Stone Age man, for instance, was transmitted in this way. The only method of transmission was imitation. Stone age boys played with implements of their own manufacture, as much like those of Stone Age fathers as possible. The continuity of Stone Age culture depended on a continuity of education by example. The development and improvement of language helped in this transmission because the example could be accompanied by an oral commentary, and so technical education was born. Although the process was thus improved, the effective transfer still depended on the reception of these cultural heritages by suitable pupils, as transmitted by suitable teachers. Any break in this flow caused a break in the continuity of the culture. Eventually, greater continuity was achieved in Egypt and some other places by the invention of writing, and so some of the records were reduced to a more impersonal and permanent form. Moreover, these ancient people became so enthusiastic about the new methods of preserving their records that they often literally covered their public buildings with them. They had found a way to a new kind of immortality, and at the same time an escape from one of the hazards of history—the break in the flow of pupils and teachers.

But the process of keeping such records was laborious. Stones were heavy and hard to cut; clay tablets were awkward if too numerous and easily mislaid; paper was scarce and perishable. There was a struggle among these materials and the verdict went to paper and ink. Because of its perishability, preservation now depended on preserving and recopying the frayed copy. For this labor much leisure and a certain amount of wealth to defray the costs were required.

Too often these were not available, and the pressures of other interests relegated these books to closets and attics, where they no doubt frequently became nest-building material for contemporary mice, as happens so often to our own literary heritages.

However, with the invention of printing, the number of books could be greatly increased. Likewise, the standard of wealth required to possess books was thus reduced. Moreover, books could be written and printed in the vernacular, and so we find the books of Martin Luther the "best sellers," in the first century of the printing press in Germany, find them read by many peasants, and find them translated and circulated in other countries. Not only had the continuity of transmission been improved, but the stream of knowledge was now greatly broadened and deepened, and for propaganda purposes the written word could now replace the spoken word. Men suitably placed spent years in study and thought and recorded their progress in books that were given to the printer instead of to the copyist. So it was in the sixteenth century.

In the seventeenth century in England a group of thinkers and experimenters, who appear to have called themselves the "Invisible College," hit upon the idea of recording in writing, and then in print, individual steps in advance in their individual fields of interest, and of publishing this material as the *Proceedings* of their society, which by this time was called the Royal Society. In some other countries the same or a similar idea was worked out about the same time. Thus the modern so-called scientific paper came into existence.

This was a clever utilization of an opportunity by which the lag in the transmission of knowledge and ideas from one receptive mind to the other was diminished. Moreover, vast amounts of time could now be saved by the diminution of the duplication of effort. Moreover, missing facts and ideas could be obtained from the perusal of these progress reports, which we call scientific papers.

Of course, all these developments accelerated progress, but it must not be thought that these were the most important factors in the progress itself. They were essentially outward expressions of a corresponding inward change in man himself. This change was essentially merely an improvement in the utilization of his mind, in wrestling with his own limitations and the forces of the world in which he lived. He had somehow obtained an excellent brain and now he was learning to use it. He had not only learned how to save and transmit his arts and crafts from one generation to the other, but he had learned how to spread the benefits over much of the population, *i.e.*, had democratized them. Whereas before these higher cults had of necessity been reserved to the privileged few, they were now accessible to the many. Although the aristocracy of learning was breaking down, the world as a whole was richer for it.

Even so, this highly perfected technique for maintaining the continuity of transmission of culture, and for spreading the opportunity of achieving a knowledge of the heritage of the past, sometimes failed to function, or in any case breaks in the continuity occurred. Nearly every one knows such instances, and they must have been more numerous in antiquity. Just one instance of this sort may be cited as an illustration. The early members of the Royal Society, and Robert Boyle especially, worked with the gas oxygen in such an intimate way that it seems incomprehensible that they should not have discovered it, and yet its discovery was delayed 100 years longer. It is true that its eventual discovery

arose from the continuing influence of the writings of Boyle on the development of chemistry, but it did not come directly out of these early experiments on combustion and respiration, as it seems that it should have. Such breaks in the continuity of transmission and therefore of progress still occur too frequently.

Our digression to this subject of the transmission of cultural achievements was made in order to show by a simple example how man has learned to capture, preserve and transmit experience. Our digression showed us that he has done this by adding the inventions of new generations to the achievements of his ancestors. We traced the process far enough in the field of communication to see how these arts have continued to grow and develop. At each stage the received knowledge was sifted, sorted, edited and organized for the new age. This reworking naturally led to the discovery of lacunae and opportunities for improvements. By the investment of great pains and patience man learned how in some respects to stem the tide of the infinite and inevitable extinction of separate and individual achievements. The life of an individual could now be more than merely the biological transmission of life to a new generation. By this method man as a race could lift himself to heights of mastery over his environment, that are high above anything that the average man as an individual could achieve by his own strength in his own lifetime.

In this connection it is interesting to note that man has developed institutions as fast as needed, whose special purpose is the preservation and transmission of this inherited and newly acquired knowledge and skill. Among the most important of these are the schools of all degrees. How well or how badly these institutions have fulfilled their function at various times and in various directions is not to be considered here: they are the most effective device that society has as yet contrived for this purpose.

This, then, as we have seen above, is the method of science: the sifting, sorting, editing, organizing and appraising of the cultural heritage by which simplification and clarification is achieved and at the same time inaccuracies and imperfections are revealed. This method provides work for every sort and degree of worker, including the gifted and luminary genius. The scientific method is, therefore, a democratic process, as distinguished from the more purely intuitive method of the isolated genius, who gains his following by the force of his own personality and intellect, *i.e.*, by his genuine aristocracy. The latter method depended on the power of a personality and the enduring loyalty of disciples. It has given the world great things in many times and places, but almost as many great things have been lost or corrupted because of the failings of disciples who lacked the genius and insight of the initiator. There can be no question that from the standpoint of individual achievement the latter method has furnished very great examples, perhaps greater than the former, but the rich fruits of modern science have been largely gained by the aid of similar giants working within the boundaries of the scientific method. We need to mention only Copernicus, Newton and Darwin as examples; men of outstanding beauty of character and democracy of spirit.

Of course, it may be objected that this accretional process is not the method of science. Such considerations are based on the idea that this method was created within the field of science, for its own special purposes, as a program for the conquest of our material and energy

environment. It is true that it did become this eventually in the days of Galileo or thereabouts and that the idea in this form then rapidly spread from the field of physics to that of physiology, chemistry, etc., but it had had a long history before Galileo.

The method of science in this modern form is the purposeful marriage of skilled hands and skilled minds, and this marriage had been consummated even in antiquity. The great civilization of Babylon seems to have been the fruit of a great technology of hydraulic engineering and irrigation. That of Egypt was achieved in geographic isolation because of the opportunity presented by the Nile valley. Imhotep was grand vizier, high priest, chief physician and chief architect of King Zoser. He it was who designed and built the first great pyramid. Here we have technical and theoretical skill of the highest degree combined in the same individual, and it is no accident of history that through Greek contacts he has been preserved to us as one of the two traditional semi-deities of medicine. The Greeks, however, for some reason did not take over this practical aspect of Egyptian culture so fully. They therefore did not properly appreciate their own Archimedes, who was no doubt their greatest scientist. This aspect of science faded out still more as Greek culture was transmitted to Rome.

In fact, it seems to have been separated gradually into philosophy on the one hand and the crafts on the other, although there undoubtedly were always some men who represented both. Thus we see Leonardo da Vinci looming up as a master craftsman and as a prophetic scientist in an age when Galileo and his discoveries were still undreamed of.

The above is enough to indicate that the method of modern science is really merely the adaptation of an ancient method to a limited set of problems that come within the boundaries of what is traditionally called science and that the method does not suffer from the limitations imposed upon it by that terminology, either in its history or in its possible applications to other problems in our day.

In order to see more clearly how powerful this method of patient and painful accretion is, let us go back about a hundred years in the history of medicine. At this time medicine had enjoyed a series of great triumphs. It had early in the sixteenth century broken the bonds of classical tradition and studied anatomy for itself. It had incorporated with itself, in the time of Harvey, the dynamic conceptions of physics and in this way made its anatomy over into physiology. It had become dissatisfied with traditional therapy and through Paracelsus filed a protest, which later gave rise to chemistry, and eventually all the riches that this approach has given to medicine. The microscope even in its crude form was taken over by medicine and was developed into a powerful tool.

At this time, namely, a century ago, medicine was fresh from such great advances, and in the midst of developing new ones. A naive questioner at this time might have asked whether, in view of all these things, the great scourges of man—typhus, typhoid and yellow fevers, smallpox, bubonic plague, diphtheria and the horrors of famine, for instance—could ever be controlled. Even the most optimistic would have hesitated to say that these and the rest could be placed under control in a hundred years. More likely he would have fallen back on pious statements and have declared that these and other calamities pursue man because of his sinful ways, and could only be removed in so short a time by divine intervention on behalf of a better race of men.

But the hundred years have passed, and we may look at the picture. The microscope gave unbelievable advances; the cell doctrine came out of it, and all that it led to. In short, an overwhelming advance in many directions ensued. The new chemistry grew and gave undreamed-of knowledge to medicine—an advance as great or greater than that achieved by the microscope. Continual reappraisal of values and concepts occurred all along the line. All the scourges named and a good many others were conquered. An infinity of knowledge of overwhelming bulk had been gained and mastered, sorted and organized, by adding together the labors of a few fruitful and inspired hours of a number of gifted workers, and the routine toil of thousands of less gifted workers, in the temple of knowledge.

There is, however, another rather astounding aspect to this story of medical advance, and that is that once it had been learned how to control these scourges, the benefits were to be applied to all the members of the population. Here it was realized that the privileged and enlightened members of society could only enjoy security as long as they successfully spread the benefits of their privileges and enlightenment over the entire social group. Thus it was realized that security lay in democracy and was to be obtained in no other way. It is true that the wealthy aristocrat could isolate himself and obtain moderate security, but complete security could only be obtained by spreading the benefits of the new conquests to the entire group. The history of this side of the question and the implications to be derived from it would require a volume.

There is a story in the Old Testament that Jacob "wrestled with the Lord," and we wonder what he wrestled about, and to what effect. The above story recorded in so brief a way is an account of a modern wrestling with the Lord, to good effect. We have seen how with time the wrestling has improved and the score on the side of man is mounting day by day.

If we may now return to our introduction, the statements made there appear in a new light. All the controls and limitations of action visualized there exist, and yet man has found a way of circumventing them. He has found a way of escaping the limitations set by the shortness of his life, the feebleness of his strength, the smallness of his mind and the brevity of his memory. These limitations appear to be "rules of the game" and there is nothing to be gained by lamenting them. Since the rules as laid down for man's biological existence can not be changed, he has circumvented them. He has escaped by subdividing the job, *i.e.*, by specialization of a sort and by perfecting a technique for recording and transmitting experience. With these two tricks he has gone far toward changing his life on earth. He has not changed the mode of entry and of exit—these are parts of the lamentable rules, so far as we know as yet—but he has greatly changed the conditions of his existence.

Now we used the above example from the history of medicine with a purpose. Man has really done better in this direction than in any other in developing an organized social technique for escaping the implications of his own limitations. The results obtained represented the synthesis of many contributing factors from a variety of view-points in the minds of men, that could control their fears of the unknown and by cool mastery of the facts bring order and safety out of a chaos of fear. Medical men are eventually made of the same stuff as the rest of us, and so the lesson is clear. If by taking thought these men could have done these great things with their own

puny hands and their own infinitesimal brains, we can by the same process and equipment get other results that we want, when we want them long enough and consistently enough to heap up the strivings of hundreds of thousands of men, into one great heap for the accomplishment of our goal. The story that we have viewed shows us that by this same scientific method we can learn to love our neighbors, control our lusts and learn to look up to our destiny instead of down to our animal origin.

The game of life is played according to very strict rules, only part of which are known to the players, and which greatly limit play. However, it must be remembered that rules of play promote skill and resourcefulness. Moreover, this capacity for skill and resourcefulness is just as much a part of our innate constitution as the attributes upon which our disconsolate friend has allowed himself to dwell.

Considered in this way, then, the scientific method is a safe and powerful process in democracy. It is safe because it is a process for overcoming ignorance and error and because it has provided itself with a continuity of transmission of achievements. It is powerful because it has contrived a way to organize and unify the individual strivings of generation after generation of men toward the attainment of a set goal.

If, as has been suggested by some, man were to discard this creation of his own spirit, he would be discarding the greatest thing that he has made. Of course, it is clear that a method so powerful for good may be badly used. Man, at his best, even in the use of this method is a blunderer. He learns as much or more by his failures as by his successes. No doubt we will need to retrace our steps in some directions and to strike out in new paths. But the permanent and enduring successes will be obtained by this method. In the meantime, while these mills of God grind their grist, we must accept such vision and select such leadership as seems best suited to the time and occasion, and we shall not know to the point of certainty, until the grist has been ground, whether we chose wisely or foolishly.

IN QUEST OF GORILLAS

X. CAMEROON FOLKS

By Dr. WILLIAM KING GREGORY

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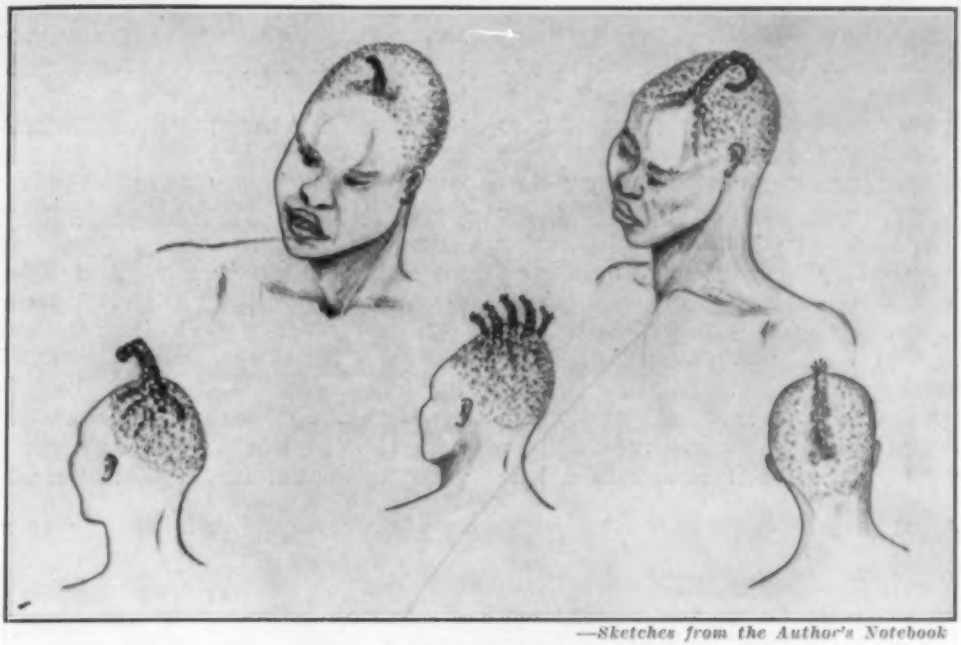
At Matadi (November 12) the Belgian customs officials refunded the greater part of the money deposited on entering the Belgian Congo and we went on board the French steamer *L'Amérique*, bound for Bordeaux and due to stop at many ports on the west coast of Africa. Our destination was Douala, the port of the French Cameroon.

Then began a most interesting four-day trip, first down the long fiord of the Lower Congo and then northwestward along the coast, stopping at a number of towns on the way. The deck passengers were mostly a lot of natives from the interior, who were being taken up the coast to work on a government railroad. Their outstanding feature was the top-knot on the crown of their shaven heads, which they had trained in a variety of comical ways, one projecting like a curved horn, another like a crown, a third like a branched horn, etc. When these men were disembarked they were picked up, eight or ten at a time, in a great rope net with their wives and baggage, swung over the side of the vessel and lowered into a large open boat. As the sea was rather rough, there were some curious scenes as each succeeding net-ful of people was dumped down on top of the earlier ones; but the whole operation was quickly and skilfully done and no one lost his good humor.

None of the various towns at which we stopped along the coast has a good harbor, and passengers and cargo have to be transferred to small boats. There is a heavy surf on the wide sloping beach, and the country as a whole is a flat coastal plain. To avoid the dangers of surf-landing, long steel piers are built out in some towns, high above even large waves, and passengers and goods are hoisted to and from the tenders which go out to the ships. In some places the blacks who propel the small boats jab the water with short paddles that end below in a wide three-lobed shovel and look somewhat like ducks' feet in action. One town (Port Gentil) exports an enormous amount of huge logs from the forest up-river, and we were surprised to see here great numbers of fast motor-boats that run up and down the river.

By no means the least interesting sights were the native lady friends of the resident white men. In wide contrast to these chattering birds of gay plumage was a French lady, a bona-fide passenger on our steamer. Although somewhat short, her features were statuesque, even commanding, and from her flashing eye gleamed instant wit, high courage and keen intelligence. Somehow she seemed to belong in a scene of the storming of the Bastille.

After a pleasant four-day trip along the coast we were transferred to a smaller boat that took us up the long estuary to Douala, the seaport of the French Cameroon. Meanwhile we had passed the mountain on the island of Fernando Po, with its summit wrapped in rain clouds. Here an enormous amount of rain falls during the rainy season. According to official reports, in 1913 at ninety-eight stations in this district a



—*Sketches from the Author's Notebook*

ASSORTED TOP-KNOTS

rainfall of no less than eleven meters (35 feet, 9 inches) was revealed for that year. At Douala in 1912 the rainfall reached 4.625 m. (about 15 feet) in two hundred and four days of rain, but this may have been an exceptionally high record. The rainfall is much less in the interior. Fortunately we arrived in Douala (November 7) near the end of the rainy season and by the time we reached Yaoundé in the interior we had dry weather most of the time.

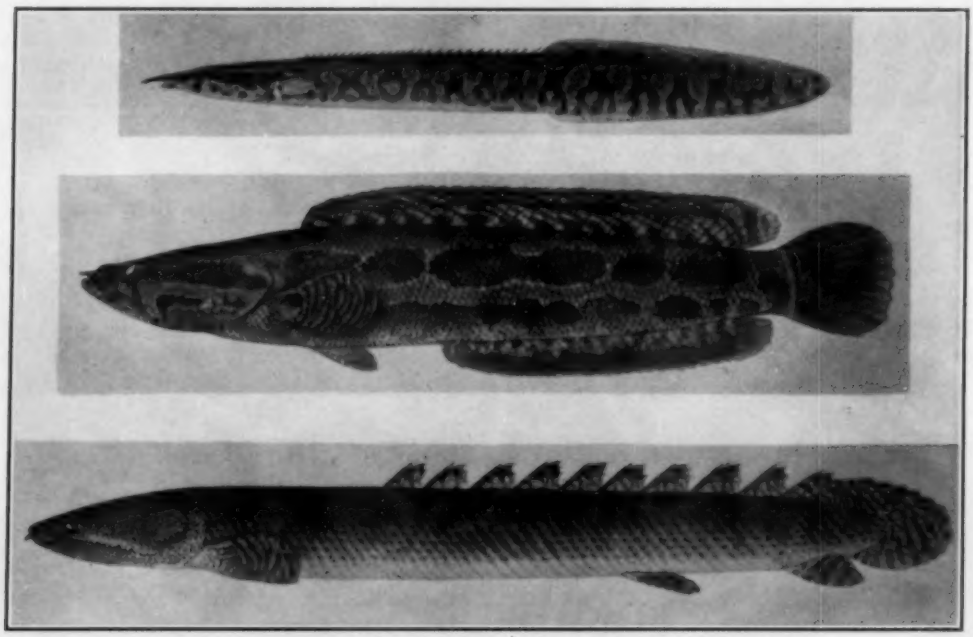
Douala is quite a flourishing city, with a gay social life. The fashionable hotel is apparently an affair of cardboard and plaster, with leaky roof and little or no water in the faucets. It has a motion picture annex in which one can see American films five years old. The streets swarm with native Africans of assorted mixtures, among whom, if we had but known how to distinguish them, we might have recognized occasional representatives of the redoubtable Fans and other invaders from the north, who in times past stewed and ate the peaceful Bantus of the forest.

The city has fine city parks teeming with African trees and fruit-bats. From the high plaza opposite the harbor one sees the towering Mt. Cameroon, 13,500 feet high, and on one side low flats upon which mud-skippers (*Periophthalmus*) may be found among the mangroves. The fish fauna in the market is chiefly marine, but in a brook near the market boys were catching snake-headed fish (*Ophiocephalus*), in which the head and the color-pattern are strongly suggestive of a snake.

From Douala we went by rail to Yaoundé, a day's journey eastward, to the seat of the Governor-General, M. Marchand. On this railroad journey we found ourselves in the westward extension of the Congo forest. We went upgrade into low mountains composed of schistose rocks and over an extremely high railroad bridge spanning a deep valley; this bridge and indeed the rail-

road itself had been built by the Germans during their occupation of the territory before the world war. The building of such railroads in tropical Africa has frequently cost heavily in terms of the lives of the Negro laborers, who, after being brought from long distances in practical slavery, have been crowded together along the narrow track and have been weakened by forced labor, insufficient food and extremely insanitary conditions, so that they proved to be easy prey to contagious diseases and epidemics.

A section of the road had recently been washed out by the heavy rains and we had to get out and walk over a long stretch of mud and sharp rocks for perhaps a fifth of a mile to reach a waiting train in front of us. All our luggage had to be carried on the heads of porters, and we appreciated the courtesy of the railroad officials, who had allowed us to ship our baggage over the road in spite of the interruption of service. At another break in the road, requiring transfer of ourselves and baggage, a slight comic relief was afforded by the black trainboy, who placed upside down a short flight of wooden steps which was intended to facilitate our descent from the high floor of the baggage car in which we had been riding. His mistake was quickly noted by the white train conductor, who



SERPENT-HEADED AFRICAN RIVER FISHES

AFTER NICHOLS AND GRISCOM, FROM COLOR DRAWINGS BY JAMES CHAPIN. *Upper figure:* THE "SPINY-FINNED EEL" (*Mastacembelus*). *Middle figure:* SNAKE-HEAD (*Ophiocephalus*). *Lower figure: Polypterus*, THE "LIVING FOSSIL." RELATIVES OF *Mastacembelus* ARE FOUND IN SOUTHERN ASIA; *Ophiocephalus* HAS RELATIVES IN INDIA AND THE FAR EAST, FROM WHENCE THE FRESHWATER FISH FAUNA OF AFRICA MAY HAVE BEEN DERIVED; *Polypterus*, ALONG WITH ITS ELONGATE RELATIVE *Calamoichthys* IS FOUND ONLY IN THE CONGO BASIN AND ADJACENT STREAMS; IT MAY BE A DESCENDANT OF ONE OF THE FISHES OF THE TRIASSIC LAKE CONGO. ALL THREE TYPES HAVE ACCESSORY BREATHING ORGANS IN ADDITION TO THEIR GILLS.

—*Photograph by the author*

A QUIET GAME OF JACKSTRAWS

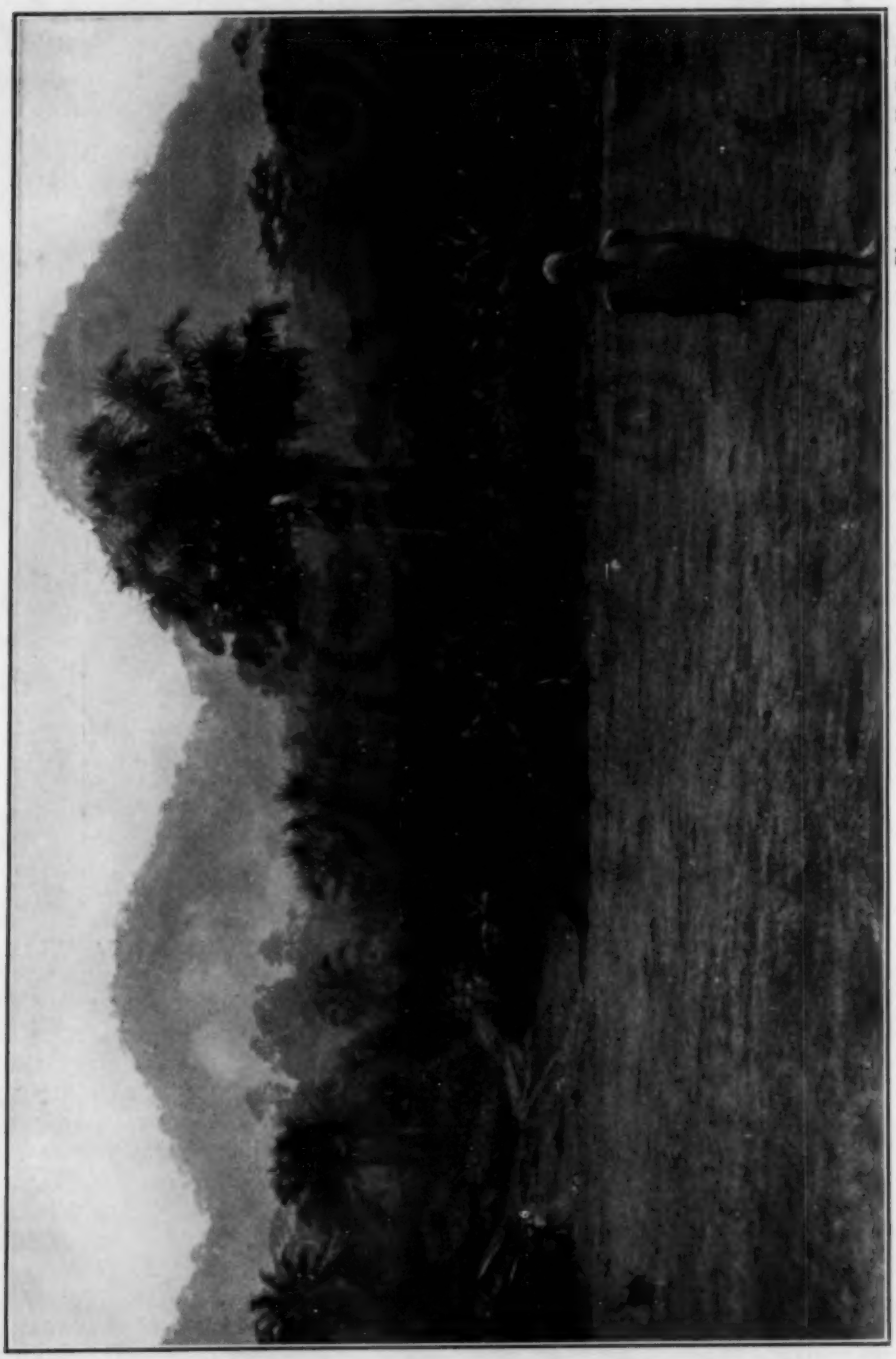
MISSION BOYS AT YAOUNDÉ.

—*Photograph by J. H. McGregor*

RESIDENCE OF THE HEAD OF THE PRESBYTERIAN MISSION AT YAOUNDÉ

kicked the poor black while emitting quite a lot of abusive language. This was in line with the frequent tendency of low-class whites to abuse the blacks, although the higher officials and the government itself tries to protect them against this abuse.

At last we reached Yaoundé, on the evening of Armistice Day, November 11, 1929. There is a fine new hotel building here, but it was not yet open and we had to go to a restaurant and dance pavilion for our supper and lodging. There was only one room available and unfortunately just outside our door a full-sized military band in uniform was providing the music for the Armistice Day Ball. It was interesting to note the absolutely correct rhythm of these blacks, who were playing old-fashioned dance tunes, but as the walls of our room were made of thin boards the volume of the music was terrific, especially as the ball kept up until perhaps 3 A.M. There were two dancing floors on different levels, the upper one for whites and the lower one for blacks, the bandstand and our room being located between them. Neverthe-



HILLS NEAR OZOUM —*Photograph by H. C. Raven*

—*Photograph by J. H. McGregor*

THE PLAZA AT OZOUM

less, we were able to sleep, at least after the ball.

The next day we paid a visit to the American Protestant Mission at Yaoundé and were delighted to receive an invitation from Mrs. Johnston to stay at the mission and make it our official headquarters during our entire stay in the country. The mission includes a fine large church in brown or buff-colored brick, a number of school buildings and a very pleasant residence looking out over a valley toward the mountains. Under the benign influences of the mission the people round about seem to live in peace and contentment. Although ordinary hymn tunes, as sung in America, are sometimes whining and droning caricatures of music, those sung by the black men at the early morning service at the mission were charged with the strange magic which Negroes seem almost always to impart to vocal music.

The mountains around Yaoundé appear to be of metamorphic rocks and are low but rather steep and hump-like.

When we paid our official visit to secure our hunting permit, Governor-General Marchand received us most courteously and after learning the purpose of our expedition, granted us a permit to secure three gorillas and three chimpanzees. He also considered very carefully with us the relative advantages and disadvantages of various localities where gorillas were known to be more or less

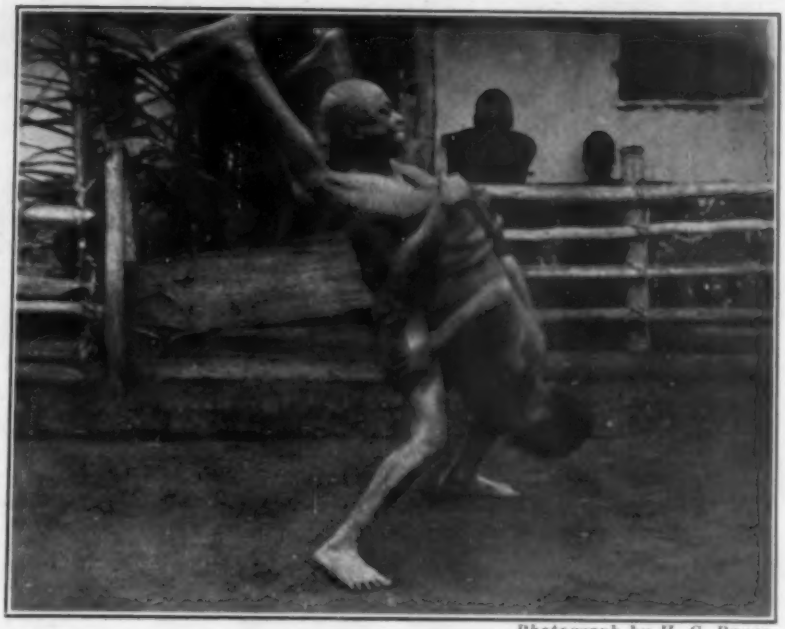
—*Photograph by J. H. McGregor*
CHIEF MARTIN ATANGANA

abundant, and he referred us for additional information to M. Canari and other government officials. It was finally decided that we should try first the district centering around the village of Ozoum, about fifteen miles northwest of Yaoundé, where gorillas had been reported recently. An official message was dispatched to Chief Martin Atangana of Ozoum, requesting him to furnish men for us and to assist us in every way. Accordingly, two days later we departed by camion in the direction of the village of Ozoum, and at a small village where the path to Ozoum joined the automobile road we left the camion and proceeded on foot up the mountain path, under the guidance of Chief Martin Atangana, his brother and their men.

Here then we were at last in the interior of a West African country and within the range of the classic gorilla of Paul du Chaillu. The rainy season was about over and the whole country was beautiful, with low humped mountains covered with forest or jungle. Most of the forest, however, was of the second growth type and there had been a great deal of cutting, clearing and planting. The population seemed very numerous and one wondered how there could be many gorillas in such a country. The people did not differ much in appearance from the Bantu peoples we had been seeing on the Lower Congo River. The villages were essentially similar also, although the houses were now made almost exclusively from the oil palms; these trees were excessively numerous, bordering the automobile roads and dominating in many other places.

As we went up the mountain path we came into some very beautiful stretches of forest with enormous trees, on some of which the leaves had turned red. Except in these small patches of forest, which were rapidly being cut down, villages of low huts straggled along the path all the way of our hour's walk from the automobile road up to the village of Ozoum. There was very little to be seen in the way of either household goods or live stock. The women were probably out working in the "gardens," which looked like jungle to us. The hard mud on the paths, by alternate wetting and drying, had taken on a polygonal pattern somewhat suggestive of basalt columns but on a minute scale.

When we arrived at Ozoum, Chief Martin Atangana conducted us to his very comfortable house and placed at our disposal two large rooms for our dining room and bedroom. Our three new boys,



—*Photograph by H. C. Raven*

TURN-VEREIN TRICKS

—*Photograph by J. H. McGregor*

SOME OF MY BOY SITTERS

whom we had engaged at the mission in Yaoundé, soon proved their ability by setting up our cots, unrolling our bed-rolls and quickly getting lunch. The chief's house was at one end of the rectangular village plaza, opposite the Catholic church at the other end. The houses of the leading citizens were on either side of the plaza and behind the houses were "gardens," chiefly of banana plants. Low hump-like mountains were to the northwest and the forest to the southeast.

—*Photograph by J. H. McGregor*

NUMBER ONE NURSE BOY

McGregor and I remained at Chief Martin's house for several weeks, while he and his men accompanied Raven in the hunts for gorillas. There were several bands within ten miles of Ozoum, and Raven and his black assistants pursued them with the greatest persistence for many days at a time and through very rough, mountainous country. At night they would stop at some native village, Raven's boys setting up his cot and unrolling his bed-roll in the house of the chief. At times Raven would send back a messenger to get something from us at the base camp; and twice he came back himself to rest for a day before starting out in another direction.

He found that the roving bands of gorillas were highly elusive and almost impossible to overtake. At one place they had raided a banana plantation, tearing down the banana plants and breaking the stout stems to get at the pith. He arrived at this place early in the morning, less than an hour after the gorillas had been there, but after following the band the whole day, up hill and down dale, he had not overtaken them at sundown. At one place the natives had caught a young gorilla in a snare set for a wild pig. They heard the screams of the young animal and hastened down there with their spears. When they arrived they found the "father," as they said, trying to pull the young one out of the snare; but the adult became frightened by the uproar of the natives and went off, abandoning the little one to its fate. The natives cut off its head and hands with their machetes and just before Raven arrived

had killed it. Raven told them that if they had kept the young one alive, they might have sold it for a thousand francs. He brought back the head of this specimen and preserved it for our collection.

The native chiefs were willing to undertake a grand drive and slaughter of the gorillas, partly in revenge for the damage done to their plantations, but Raven of course would not authorize or take part in such a procedure. In preparation for such a drive the natives would spread many nets in certain places and try to drive the gorillas toward the nets. They could then spear and hack the animals while the unhappy creatures were struggling to disentangle themselves.

The eating of men being out of date, the natives find the gorilla a good substitute, and a certain "white hunter" in Douala told us that he had often been employed by the government to kill elephants and gorillas as food for the blacks that work on the railroad. From Raven's experience in this and other places in the interior of the French Cameroon, he concluded, however, that perhaps ten times as many gorillas are killed by the natives themselves as are killed by white men.

Meanwhile McGregor and I had been having a great opportunity to become well acquainted with the people of the neighborhood, and McGregor secured a large series of cinema and still photographs of them. There we were again impressed by the wide range of variability, both in physical appearance and in disposition of our dark-skinned friends. Chief Martin Atangana was a tall, well-proportioned man, with large eyes and a full beard. In general beards seemed better developed in this region than in the Kivu district, possibly owing to admixture of the forest Negro stock with invaders from the north. Like many of the older people, Chief Martin could remember the time when the Germans came into the country and he had been taught to speak German well in the German Catholic schools. He was a sincere and consistent Catholic, not only

From the Author's Notebook

SKETCH OF PETULIS

THE FEATURES ARE PROBABLY ALL SOMEWHAT TOO LARGE AND THE CHIN TOO SMALL.

—From the Author's Notebook

SKETCH OF ELUNA LUMBELE

reading a German devotional book aloud every day but cooperating with the present French priest in maintaining the discipline of the church in the village.

Not a few of the men of the village were tall, stately and heavily muscled, like some of the fishermen at Stanleyville. Many had massive skulls with a median elevation and a flattened area on either side of it (scaphocephalic type). The nose was usually very broad at the base and there was often a bony bridge above the nose and between the eyebrows. The mouth was large with protruding lower lip and cupid's-bow upper lip. But along with these and other conspicuous negroid features there was an immense range of individual variability. Some men, for instance, had more or less mongoloid eyes, with a perfectly developed "Mongolian fold" covering the inner corner of the eye. Wherever it occurs the presence of this feature in adults is due to the retention of a character that is normal to mankind in general during foetal life but which usually disappears before birth in whites. The presence of this feature in Negroes is probably no sign of admixture with mongoloid elements. The face as a whole also varied, one man having the grave lineaments of a statesman, another the comic face of a clown, while a third was just a lethargic peasant.

We did not have the opportunity to study the features of nearly so many women as men, for the reason that in general the women had to work in the gardens every day and also fetch the water, do the cooking and other household work (or what there was of it), so that the men could absorb the proper number of sun-units each day and settle the affairs of state in generous palavers in front of the chief's house.

Our greatest opportunity, however, was to become acquainted with the numerous children of the village. The evening of the day that we arrived the little boys began to perform for us on the open plaza in front of the chief's house. Some hopped like kangaroos, others did elementary "turn-verein" stunts, which might perhaps be reminiscent of tricks taught to their grandfathers by the Germans.

A very charming feature of African villages in general was that the little boys were permitted to play about in the plaza and enjoy themselves without repression from their elders. Neither the heathen nor the Catholic Negroes showed a suggestion of the dour medieval Puritan view-point that children must be lashed and bullied into the way of salvation. Doubtless the initiation rites of the heathen blacks in many districts have been horribly cruel, but so far as I could see, the young boys and girls in the Kivu mountains, in the Congo forest and here in the French Cameroon, were free to play and frisk about whenever they were not in school or carrying the baby.

We possessed a number of articles that soon made our house a center for the children of the vicinity. McGregor had brought with him a pair of trick spectacles, to which were attached large "googly" eyes that would jiggle in a comical way when the wearer shook his head. All the small boys and a good many of the men enjoyed trying these on and making the others laugh. Then the children never tired of looking through our field-glasses, although most of them preferred to look through the wrong end, which gave a reduced but very sharp image of the boys near by. On dark nights we let the children look along and above the tube of our flashlights at the eyes of the sheep in the plaza. Such delighted "Eeeees!" and "AH KEHs" as each one caught the dazzling green flash from one pair of eyes after another! The men and women were just as eager and friendly as the children. McGregor

—*Photograph by J. H. McGregor*

THREE LITTLE MAIDS FROM SCHOOL

NGA HEADS THE LINE.

would sometimes cut out little paper figures of sheep, horses and goats and color them with crayons and these were much sought after.

But perhaps the most unfailing amusement was that of crowding around and looking over my shoulder as I struggled to record in my notebook a sketch of some boy or girl who was sitting more or less patiently in front of me. When the onlookers got too boisterous I would suddenly wheel around and hiss at them in a stage whisper, "Hush! keep quiet!" at the same time raising both hands and fluttering them in the manner of an orchestra leader trying to signal for a *pianissimo*. This would start much ill-suppressed merriment and several of the boys would repeat the gesture, hissing "CHIP KYATT" with great emphasis. Nevertheless, they knew what was wanted and would indeed quiet down and move away from my chair—for a few minutes. And what an opportunity that would have been for a real portrait artist, who could have made five portraits while I was struggling with one.

How vividly the individuality of each of my sitters impressed itself upon me as I strained my eyes and brain to catch his elusive personality as his features changed faster than the reflections of clouds in moving water. There was Barucha, a most docile and placid little

—Photograph by J. H. McGregor

A CONTRAST

NGA IS STRONGLY PROGNATHOUS, WHILE HER FRIEND HAS A STRAIGHT FACE. OUR HOUSE IS SEEN IN THE BACKGROUND.

savage of pure Negro type. There was Détus Ogmba, a broad-faced little rascal with dancing eyes. "You gee mee monee," he would say. "I gee you CRACK" I would say, hitting my own head with my knuckles, a gesture which he would imitate with monkey-like fidelity. But Détus was full of understanding and sympathy. He knew that I wanted him to sit still and so would hold the pose far longer than any other sitter.

Then there was Damien Belibi, a young nuisance of unbelievable persistence and aggressiveness. His narrow nose and small mouth suggested a rich endowment from Semitic and Hamitic conquerors. If we gave out bananas or oranges Damien always wanted all his own share and half of as many other shares as he could take away from the others; they, strange to say, seemed to submit to his aggressiveness as if he were already a chief, instead of being only a relative of the chief. Good little Sylvanus Atangana would hold his baby brother by the hour and sit there on the piazza arrayed in a great green Calladium or perhaps Papaya leaf. He was quiet, obedient and smiling, but very inattentive; I gathered that he did not have nearly as much brain as the aggressive Damien. Utterly different from all the others was Aloid Amanana, whom I called the "young ascetic" for obvious reasons. One might attribute the prominence of his bones to disease, but what gave him the narrow nose and delicate mouth except perhaps inheritance from proud Hamites?

Minye Ngama, although the son of the old "king," was a dull, heavy-faced child, who inherited his father's powerful clutch on a metal franc. Nevertheless, he was one of my best sitters, as he was too stolid to flash about like some of the other children.

Some of my friends at home, who never saw the originals, were good enough to be highly entertained by my "portraits"; and such is the magic of self-deception that I long felt that I had captured the most essential features of these elfin faces. Unfortunately, or fortunately, I recently saw an actual photograph of these same boys and the effect was devastating to my pride; for by missing certain fundamental proportions of the parts, the details which I had so carefully noted were out of scale. However, I still think that my portrait of Petulis conveys a fair idea of his general make-

up, and luckily Professor McGregor has no photograph of him to destroy the illusion.

After portraying the "king's son" my fame, or the fame of my francs, seemed to spread rapidly and I was besieged with prospective adult clients. I finally selected two of the most persistent candidates, Petulis, nicknamed the "Bonehead," and Eluna Lumbele, nicknamed the "Jolly Executioner," because he seemed to have been born for such a part. Petulis had very kindly tried to assist me in posing the models; and the fact that he invariably misunderstood what I wanted and was far more of a nuisance than a help is not set down in malice. He made an excellent model himself, however, first, because the occupation of earning a franc by merely sitting still appealed to his whole nature; secondly, because he was vain enough to admire the portrait; and thirdly, because his grand mouth and nose, sleepy, half-shut eyes and thick-topped skull were all salient features that could be recorded even by a novice.

And what a thrill I experienced when Eluna Lumbele presented himself for a portrait. He lived a little way up the road, and once when I walked by he hastened out to invite me to notice the fine new house he was building. On that occasion he wore only a loin cloth and I was impressed by his huge athletic frame, his small head and super-comic face. But to-day he was arrayed in all his dear-bought toggery, of which he evidently wanted nothing omitted. By gestures, copious language and broad smiles he intimated that I should dismiss my small boy model and begin with him. Such an opportunity was not to be lightly missed, so I quickly paid the boy his franc and sat Eluna down on the box on the back piazza. But what if the portrait should offend his dignity and he should start to mop up the would-be artist? It was getting late and I worked as I never worked before. Never had I seen such a wide nose, nor such an ample median bump on the forehead, nor more slit-like eyes, nor more decorative tattoo marks, nor a more self-satisfied ensemble. I did not dare to slight any of the tattoo marks on his face, as they were his heraldic devices, telling the world that he was every inch a Yaoundé man. At last it was too dark to go on, so I got my shaving mirror, gave him a good look at himself and then held up the nearly finished sketch. The bystanders roared approval, smiles stretched further his broad mouth and his eyes became slits. The fee of one franc made him more

—Photograph by J. H. McGregor

A YOUNG PRINCESS

—*Photograph by J. H. McGregor*

THE CHURCH AT OZOUM

happy and vain than ever. On the next sitting a few days later I corrected and finished the sketch (as far as I could). Then I suggested that he bring along his wife to view the portrait. Serious looks and dejected mien. "No got wife. Wife cost plenty, plenty francs. No got francs."

After that he was very assiduous in his attentions to me, frequently intimating that I should give him another franc and apparently hoping that I might even give him enough to purchase a wife for his new house. But I always laughed at him and made gestures signifying that he should give me francs for making his portrait.

One of my best sitters was Nga Mpuoko, a little girl of perhaps eight or nine years, who spent much time about our door. She was a remarkable little actress and could make the most comical faces with her almost ape-like lips. Also she gave an impressive imitation of the mourners at a funeral. She used to address us respectively as MACGRIGWARR and GRIGWARRÉE.

At night when all was still in the village, the sheep sometimes crowded up on our porch and wagged their tails so vigorously that our walls would shake. Large hairy spiders dwelt above the door and with a flashlight one could see the green gleam in their eyes. But we were secure under our tightly tucked-in mosquito netting and as long as the spiders seemed to be minding their own business we chose not to bother them. As to driver ants, the broad village plaza, picked clean of every blade, discouraged their invasions, although we once saw them streaming through a hut along the path.

While Raven and Chief Martin and several of his men were hunting gorillas I made several excursions in the forest near by, ostensibly in search of monkeys but really just to see the forest itself and whatever it might reveal to me. I took one of the chief's relatives, Andreas

Ysombe, as a guide. He was a tall, powerfully built man with regular features and a black beard. We wandered for several hours in the forest, which was very similar to that at Banalia, north of Stanleyville. The underbrush, thorns and vines offered only a moderate amount of opposition whenever we left the path and we seldom needed a machete. The forest was very still, except for the hubbub made by hornbills; monkeys were either very scarce or remarkably secretive. At one place Andreas warned me away from a pit which had been dug for wild pigs and antelope. Here he stopped a while and gave out a series of remarkably loud sounds, purporting to be antelope calls, but no antelope appeared. Then we went down the valley to the bed of a stream and Andreas motioned to me to climb up on his back. After I had done so he walked up the stream some distance until he found a suitable place to get through the bush on the other side. I then got down from my human steed and we came home.

On another occasion I took another man, named Benedict Nkudu, to a different part of the forest. He went through a similar performance of calling antelopes, but his rendition of the call was widely different from the other man's. Both these men could call antelope successfully, for the first one showed me many skulls and horns of large antelopes which he had called in this way, and the second afterward called two small ones successfully when he was out with Raven. On this day, however, his calls produced only an echo and we went on after monkeys. For a long time we did not see any, but after a while one or two began to leap from tree to tree. They retreated into the lowest and thickest part of the valley far more quickly than I could follow them. So I stood still and let the man go ahead. He was out of sight in a moment and I heard nothing of him for a long time, so I cleared away a few stems and sat down on them to study the forest. Pretty soon I saw a little movement in the tree and in a moment a beautiful spot-nosed monkey was creeping forward on a branch immediately above and in front of me. I kept very quiet and it was a minute or two before he discovered me and leaped away. After another short while a stick fell down out of the tree in front of me;—and there stood my guide, smiling, a little way up the hill behind me. He had used up the several bullets I had given him, but had shot no monkey. His foot was bleeding on the thick sole, where a stick or stone had penetrated it. He then cut a small switch and whipped the sole of his foot rapidly at the spot where it had been punctured until the blood flowed freely. Apparently this was a native method of preventing blood-poisoning. In a minute or two he was ready to proceed. By this time it was about ten o'clock in the morning and there was slight chance of our seeing any more monkeys until late afternoon, so we went back to camp.

The religious services of the church at Ozoum were one of the most remarkable features of the village. At matins, high mass and vespers the church was filled with men, women and children eager to take an enthusiastic part in the service. The French priest, Father Antoine, was very friendly and courteous to us and extended to Dr. McGregor and myself a cordial invitation to attend the early morning mass. Accordingly we went in to the church and took places near the door; but Father Antoine sent one of his black catechists after us and we were conducted to the front seat immediately in front of the chancel rail. As if this were not embarrassing enough, just before the time came for the people to kneel at the chancel rail, the black catechist again came to us and conducted us to two chairs which had been placed

within the chancel, between the altar and the communicants. For nearly an hour the congregation took a lively part in the service, the frequent and sometimes long responses being given with vigor, precision and in perfect unison, all in the native language. This technical perfection was the result of daily practise conducted by the several catechists even while the priest was away visiting the many other villages under his care.

The children in the school were taught to read and write in their own language, chiefly for the purpose of being able to read the words of the prayerbooks and other religious books, which were their sole and sufficient literature. To teach them this and to enforce the discipline of the church naturally taxed all the strength and time of the priest and his catechists. There was thus no time or energy left to fight the fearful diseases that afflict the people or to teach them hygienic living, this apparently being regarded as the duty of the state.

When we came out of the church I saw an enormous millipede crawling slowly along on the open plaza. I admit that it was too "crawly" for my nerves, but I called McGregor and he grabbed it instantly and thrust it into his pocket. When the folks coming out of church saw this they screamed and scattered in all directions. Doubtless our prestige, in being able to handle such "deadly" critters with impunity, must have been greatly enhanced.

One bright day in Ozoum we heard a woman begin to cry out and moan. Immediately every one stopped what he was doing and began to drift across the street to a certain house where a younger brother of Chief Martin had just died. In a short time the news had traveled and people streamed in from every direction, mourning loudly. Soon the roar of the mourners was like the hum of an immense hive. The dead man's widow, reasoning in the native way, asserted that he had been poisoned. Chief Martin requested us to go to look at the body, which we did. McGregor assured him in German that we saw no evidence of poisoning, but that the man had more probably died from pneumonia. The chief told us that many people were killed by a "worm" that gnawed its way into the lungs, and that he himself had once been ill from this cause. That evening a fire was lighted in front of the house and the mourning chorus was renewed. Late in the night the body was carried to a near-by village. The next night the drums in that village were most insistent, and as I lay in bed listening to their magical rhythms it seemed to me that they must be describing the long journey that the dead man was making in the world of shades.

A few days later the widow returned to Ozoum, making loud and dismal lamentations as she entered the village road, which she continued for some little time after entering her house. But somehow I received the impression that a good deal of the public and private mourning was more or less overstimulated and that after the dead had been decently mourned the grief of all was assuaged and life went on much as before.

During these three weeks of persistent hunting Raven was assured many times and in many villages that there were "plenty, plenty ingagi" (gorillas) near by. But although he and his black assistants followed gorilla tracks for long distances and on two or three occasions came near to one or more gorillas, he was never near enough to shoot one through the head and he stuck to his policy of not taking random shots at the body. For on all his field work in many parts of the world he has been scrupulously careful never to kill a single individual more than he has a permit for, and it

would annoy him extremely to wound an animal and let it get away. Although Chief Martin and his men were enjoying the opportunity to hunt gorillas with him and pressed him to stay on with them until he got one, Raven decided that he would try his luck in another district; so he came back to the village of Ozoum and we prepared to leave.

But before leaving we gave a little party for the children of the village, who had all been our friends for the past three weeks. McGregor and I walked down to Yaoundé, taking one of our personal boys, to purchase the material for the party. On the day before we left the native school-teacher marshalled his pupils and led them forth, chanting the school hymn. They drew up in front of our house in the presence of the chief and head men and were arranged in files of three each. McGregor and I then passed down the lines, distributing to each boy a handful of candies, a pencil, two heavy rubber bands for his bean-shooter and one franc. For the few little girls present we had candy and oranges. The whole affair was carried out in solemn silence, and we felt rather depressed at having unintentionally thrown such a wet-blanket over our usually light-hearted young friends. We returned to our porch and told the catechist that that was all. But he kept the children standing there and we retired into the house. In a few moments there was a mighty chorus: just "GOOD BYE!" but delivered with all the decision and unison of their religious services. It was not until late that afternoon that they plucked up courage enough to crowd around our piazza and beg for bananas and francs with their usual informality and persuasiveness.

Early next morning, when we and all our camp equipment passed down the mountain path, Nga and Détus and several others made the trip with us to the auto road. There we took leave of all our good friends, from Chief Martin down to Détus Ogmba, who said he was willing to go with us into the great world. And seldom have I been sorrier to part with friends anywhere in the world than with these engaging ex-cannibals of the Congo forest.

THE THUMB OF MAN

By Dr. EDWARD L. TROXELL
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We must acknowledge that with all his noble qualities, with his god-like intellect—with all these exalted powers—Man still bears in his bodily frame the indelible stamp of his lowly origin.—*Charles Darwin.*

MANY people are inclined to reject any theory of man's origin from a lower form of life because it seems to them ignoble. In their minds the evidences of such a beginning for the human race do not suggest mere humility, but rather constitute a stigma, a blot that causes a sense of shame; it is a thing to be concealed.

It is our purpose in the following pages neither to apologize for the course of events in our prehuman existence nor even to enumerate the many evidences, those stigmata, that show the trend of our ancestral development. Rather it is our plan to follow through a consecutive series of events that have, during some fifty million years, brought our race up from the earliest primates; we shall endeavor to trace the line of cause and effect that culminated in the achievement of man's highest mental and physical qualities.

In our treatment of the subject we shall put especial emphasis on certain new features, such as the development of the opposable thumb, the bending forward of the cranium, the reduction of the "animal" features of the face and the assumption of the upright carriage; but one should not lose sight of those old-fashioned characters, the scores of vestiges of once existing parts that constitute "the indelible stamp" of our humble origin.

It was at a time before the so-called age of mammals and well over a hundred million years ago, in the Mesozoic era, that certain animals began climbing trees for safety and for food; a new type of food, consisting largely of the fruits and nuts, was coming into existence at that time. At first these humble creatures climbed for their sustenance and security by using their claws just as many carnivores and rodents do to-day. Some of them ran on top of the branches, making use of their padded feet; but others learned to climb and cling by employing the first digit, the thumb, set off at an angle from the rest of the hand. This not only gave the hand a broader base, but with the thumb extended from the other fingers and opposing them the hand came to have a wrench-like grip that prevented twisting and slipping on the limb of a tree.

Out of the employment of the thumb, at first in climbing and gripping the branches of a tree, there arose other and higher uses for it, and eventually the thumb had the greatest significance in the progress of the human race. Let us follow the development and employment of the thumb, that member of the hand sometimes unwittingly held in contempt.

One should go back and trace, however briefly, the history of the animals that constitute the links in the chain of our ancestry and see what influence the structure of the hands and limbs may have had on the habits of life, the better to understand how it was possible for man, the upright one, to have been evolved. We begin with the earliest primates known in recounting this history and our minds go at once to the little *Pelycodus* of the early Eocene epoch. It has been our good fortune to find many specimens, consisting mostly

of jaws and teeth, of this lemuroid once inhabiting the region of Wyoming. While we do not know the complete form of *Pelycodus*, yet its successor, the *Notharctus* of middle Eocene, is well represented by skeletons and separate bones in the American Museum, and it has been carefully studied and described by Granger, Gregory and others of that institution. *Notharctus* is thought, by those who are competent to judge, to have had at that early time just those qualities that one would expect to find in the ancestor of man and of the higher apes.

There is good reason to believe that *Pelycodus* and *Notharctus* (Gregory, 1928) had achieved already the prehensile hand and foot, the opposable thumb and great toe, and in them the foundation was laid for that structure without which our present civilization could never have been attained.

It is difficult to say just what progress was made in the use of the thumb by the succeeding primates, *Parapithecus* and *Propliopithecus* of the Oligocene period, but it seems highly probable that the well-known genus, *Dryopithecus*, had come to use the hands in many more ways than merely for hanging to the branch of a tree for support. Here in *Dryopithecus*—tree ape—we expect an ancestor that surely had learned to use the hands in gathering food, in arranging sticks for a comfortable nest and, above all, for holding objects before the eyes for closer scrutiny. Inquisitiveness was born. Curiosity had come to be a racial trait. What an impetus this would give to the development of a higher intelligence!

While it can not be said strictly that the presence of the hand itself was a cause, yet given a brain—from whatever cause—the hand stimulated and enhanced its better use. As the hand played to the mind so the mind to the hand and they mutually stimulated fresh and higher consequence. The hand furnished an instrument for the fuller development of the mentality and those creatures that chanced to have a higher brain capacity were able to use the hand to their greater advantage. A high reward awaited the possession and further employment of both. And so it came about that for millions of years there continued a selection in nature of that species of primate in which the mental capacity gradually improved.

This happy agreement between hand and mind brought about another signal step in the upward trend of this fortunate race. For some time the superior mental equipment and the useful hand had made these primates less dependent upon the great size and strength of the jaws, upon the number, form and arrangement of the teeth, upon the sense of smell and so on; the result was that the facial portion of the skull decreased in its proportions while the cranial dome used more and more material in its construction. Less and less of jaw, except that the chin lagged behind in its reduction, restricted were the teeth in size and number, fewer and fewer were the bestial propensities; instead there came more and more of that which stood for higher and nobler thoughts and actions. Always this was accompanied by the further training of hand and thumb and the course was laid that made it possible for these primates to achieve the highest place in the scale of life.

Observe a further progressive change. The brain, because of its tremendous development in this rapidly evolving race, began to occupy not only the back of the head, as in the earlier progenitors, but the top as well and it even crowded forward upon the region of the eyes and nose as the forehead encroached on the face. The results, pointed out especially by Tilney, were of the utmost importance: the nasal region became so compressed and restricted that the sense of smell was largely lost; the eyes were pushed forward and closer together re-

sulting in binocular, stereoscopic vision. The skull was bent, even in prehistoric man, so far forward that no more could he see above his head or behind his back as did the earlier lemurs. It became virtually impossible for these creatures, standing on all fours, to see other than the ground beneath.

Now note the significance of all this. There came a time when it was necessary for our ancient forebear to stand up or sit up in order to see what was before him. In moving about he was compelled to rise upon his two feet, to assume an upright posture, and he became man. Out of a series of events that seemed destined to turn his face inexorably toward the soil, there resulted an exaltation to a higher, more noble position. In many ways, indeed, it has proved a serious business, and often certain of our primitive organs find it more than they can endure. Many human ills arise from this new estate.

So long as our ancestors remained in the trees, perhaps throughout the time of *Dryopithecus* in the Miocene-Pliocene epochs, the bending of the skull forward made no great difference. As a matter of fact it may have been, as some strongly contend, that the first trend toward the upright posture had its inception in the trees. But certain it is that just at this time the environment offered a new and impelling influence; a change in climatic conditions, well recognized in the geological record, caused a wasting away of the forests that made it increasingly necessary for those ape-like beings to drop down upon the ground, to cover the distance from tree to tree, from grove to grove, on their feet.

It was then and thus that the quadrupedal gait was discovered to be entirely out of harmony not only with the hand but with the form and structure of the skull, and *Dryopithecus*—his name notwithstanding—was forced to make his way on foot upon the ground. He found that the bipedal gait was as convenient and essential as the quadrupedal method was awkward and unnecessary. As early man found it an advantage to stand upon his hind legs to see what was transpiring about him so every newborn babe to-day must discover for itself anew this interesting and important principle and each in turn must learn to rise from its hands and feet, a quadruped, and solve again the difficult problem of seeing and understanding the world about it.

And it was a fortunate thing that our ancestors were finally forced out of the trees in the Pliocene time, if we may judge by what happened to certain of the other primates. Take, for instance, the spider monkey (*Ateles*) that has lost its thumbs entirely, or the gorilla, chimpanzee, etc., whose thumbs are greatly reduced, and it becomes evident from these examples that we too might have been deprived of the use of this most important member, had our race remained in the comfortable trees. Without the thumb the life of modern civilized man could never have been a possibility.

We review the sequence of events:

The thumb separates to give broader "footing."

The hand learns to grasp the branch of the tree.

Useful as a tool, the hand stimulates the mentality.

Result, increased capacity and power of the brain.

Bone from the face goes to enlarge the cranial roof.

Nose, jaws and teeth decrease in size and function.

Forehead crowds the eyes to a place of greater vantage.

The eye and hand still more faithfully serve the brain.

The face bent forward forces man to an upright posture.

Bipedal gait frees the hands to further assist the brain.

Upright man reveals the remarkable evidence of his progress.

But still shows the "indelible stamp" of his humble origin.

THE CREATIVE YEARS IN SCIENCE AND LITERATURE

By DR. HARVEY C. LEHMAN

PROFESSOR OF PSYCHOLOGY, OHIO UNIVERSITY

WHAT are man's most creative years? At what ages are men likely to do their most outstanding work? In 1921 Professor Robert S. Woodworth,[1] of Columbia University, published this statement in his book, "Psychology: A Study of Mental Life":

> Seldom does a very old person get outside the limits of his previous habits. Few great inventions, artistic or practical, have emanated from really old persons, and comparatively few even from the middle-aged . . . The period from twenty years up to forty seems to be the most favorable for inventiveness.

Writing under the fictitious name, Helen Nelson,[2] an author whose article appears in the *American Journal of Psychology* takes vigorous exception to Woodworth's foregoing assertion. After arguing at length against Woodworth's statement, Miss Nelson lists a number of individuals who did notable work after they had reached the age of forty. Presumably upon the basis of her citations, Miss Nelson arrives at the following conclusion:

> . . . in the case of these names at least, invention of the highest order, far from being in decay at forty, seems to be at the very prime or just ready to begin.

The *method* by which one arrives at a conclusion is no less important than is the conclusion itself. For example, if a writer or a speaker should attempt to describe seriously incidents that were alleged to have occurred on the opposite side of the moon, the critical listener would (or should) immediately ask *how* the speaker had obtained his alleged information. And, if queries regarding the problem of technique could not be answered definitely and to the entire satisfaction of the interrogator, intelligent auditors would probably see no reason for taking the speaker's remarks very seriously.

The same principle should hold for reports which deal with human behavior. If credence is to be given to research findings it is always important to know *how the alleged findings have been obtained.* Certainly, the serious student is entitled to know whether the research worker was prejudiced or whether, like the expert athletic official, he was able to view with an attitude of calm detachment the phenomena which he claims to have observed.

It is one thing to set out with a ready-made conclusion and to search for illustrations that may be employed for the purpose of proving its validity. It is quite another thing to assemble factual data and, while doing so, to maintain an impartial attitude. In the article previously referred to, Miss Nelson seems rather obviously to have employed the first of the above-mentioned techniques and, writing in an inimitable literary style, she proceeds (by the selection of some exceptional cases and the omission of others) to defend her belief that old age is the most favorable time for inventiveness.

Literary skill and the ability to persuade are not valid substitutes for sound research technique. With this elementary principle in mind let us see what is

[1] R. S. Woodworth, "Psychology: A Study of Mental Life," p. 519. Henry Holt and Company, 1921.

[2] Helen Nelson, *The American Journal of Psychology*, 40: p. 304, 1928.

to be found when the inductive method is employed as the means for studying "the creative years." We, of course, shall not attempt to canvass all kinds of endeavor. Such a large undertaking would be impossible of accomplishment. Let us therefore limit our inquiry. Let us first examine the field of creative chemistry and attempt to answer the following question. Do chemists display more creative thinking at some chronological age levels than at others?

In his book, "A Concise History of Chemistry,"[3] Professor T. P. Hilditch, of the University of Liverpool, presents the names of several hundred noted chemists and the dates on which these chemists made their outstanding contributions to the science of chemistry.[4] Professor Hilditch's book was chosen for study for several reasons. In the first place, Hilditch is an able worker in the field of chemistry and his institutional connection is above suspicion. It obviously would have been an absurd and a wholly indefensible procedure for the present writer to have attempted to evaluate the work of noted chemists.

It was stated previously that Hilditch is an able chemist. But even this fact did not satisfy the present writer. Hilditch's judgment might be after all "only one man's opinion." Professor Hilditch's book was submitted therefore to four different university teachers of chemistry. These four chemistry teachers were unanimous in their judgment that the contributions listed by Hilditch form an essential part of the chemistry panorama. On the basis of the collective judgments of these advisers, the writer concluded that Hilditch's list is probably a fair and adequate sampling of outstanding achievement in the field of creative chemistry.

There is a second reason for utilizing the list of achievements that was set forth by Hilditch. When he was selecting important contributions in the field of chemistry, Professor Hilditch was *not* studying age differences in creativity. He was probably therefore not even aware of age differences. This fact is of considerable importance in so far as the present study is concerned. Certainly, when studying the creativity of persons of different chronological ages, an investigator should divest himself of all bias for or against any particular age levels. And this impartial attitude is most likely to be attained when the compiler of a list of outstanding achievements is thinking *not* of age differences but solely of outstanding achievement.

If each age group is to be judged fairly it is necessary to take account of a number of other things. For example, one must consider the number of individuals that are alive at each successive age level. Certainly, if more men are alive at the younger age levels than at the older age levels, the younger age groups might achieve more merely because of their greater numerical strength. Adequate allowance for the unequal numbers of individuals alive at the various age levels was therefore made in a manner that will be hereinafter described.

One final word with reference to the problem of technique. It obviously is not possible to study the entire life work of individuals who are still living and achieving. In the first place, it is almost impossible to judge the real significance of quite recent work. Moreover, we have no way of knowing what the living chemist will accomplish during his later years. The present study includes therefore data for deceased chemists only.

[3] T. P. Hilditch, "A Concise History of Chemistry." D. Van Nostrand Company, 1911.

[4] Subsequent personal correspondence with Professor Hilditch elicited the following statement regarding the dates of achievement: " . . . the dates given in my history as those in which contributions were made are simply the dates of first publication of the results in a scientific journal or similar medium. In general I would say that these dates would be within a year of the time when the discovery, etc., was actually made. The interval might be less than this, but would rarely be more."

For the deceased chemist the record is reasonably complete and future events will probably change the record only slightly.

In so far as data were available, the birth and the death dates of the chemists listed by Hilditch were secured from histories of chemistry and from various other sources. With the foregoing information it was possible to determine the chronological ages at which the world's most renowned chemists made their most significant contributions, both theoretical and experimental, to the science of chemistry. The findings are set forth in graphic form in Fig. 1.

Fig. 1 presents the chronological age levels at which 244 chemists (now deceased) made 993 significant contributions to the science of chemistry. In Fig. 1 the histogram (the line containing many right angles) presents the data by yearly intervals; the polygon (the curved line) presents the same data grouped by five-year intervals. In studying Fig. 1 it should be borne in mind that this figure presents the *average* number of chemical contributions per chronological age level. Full and adequate allowance is thus made for the larger number of youthful research workers.

For example, it was found that at age twenty-nine, 244 chemists made 49 contributions. This was slightly more than an average of .2 of a contribution per individual. Of the 244 chemists that were alive at age twenty-nine, 220 were living at age 50. The 220 workers that remained alive at age 50 made only 23 contributions, which was slightly more than .1 of a contribution per living chemist. In Fig. 1 the histogram is so drawn as to be only about half as high at age 50 as at age 29. The histogram is drawn in this manner in order to show graphically that the *average* number of chemical contributions per living individual was only about half as large at age 50 as it was at age 29.

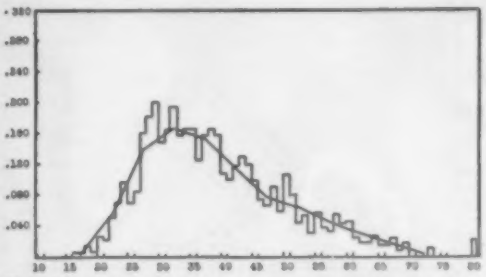

FIG. 1. AVERAGE NUMBER OF CONTRIBUTIONS BY CHEMISTS IN EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 993 SIGNIFICANT CONTRIBUTIONS THAT WERE MADE BY 244 NOTED CHEMISTS.

If, regardless of the number of workers that remained alive, the fifty-year-old chemists had contributed *at the same average rate* as did the twenty-nine-year-old chemists, the curve in Fig. 1 would have remained as high at the fifty-year-old age level as it is at the twenty-nine-year-old age level. Actually, the curve in Fig. 1 exhibits a very noticeable descent at the older age levels. Why does this marked descent occur?

A copy of Fig. 1 was mailed to Professor Hilditch. In commenting upon it, Professor Hilditch wrote as follows:

> Frankly, I am surprised that your analysis of the data leads so clearly to the age range of about 30 to 35 being the most productive for chemists. One cannot, of course, dispute the accuracy of a mathematical analysis of this kind, and yet the results are contrary to what one would have anticipated.
>
> I am not surprised that the maximum productivity should appear by about the age of 30, but I would have expected this maximum to be more or less sustained over the succeeding 20 or 25 years. I think it would be generally agreed amongst chemists that the productive type as a rule continues to develop and *mature* until well on into what is generally called the prime of life.

The present writer requested several friends to explain why the curve in Fig. 1 rises so much more rapidly than it descends, and why it descends so quickly after reaching its maximum. One individual suggested that the older chemists may have become more discriminating during their later years and that the

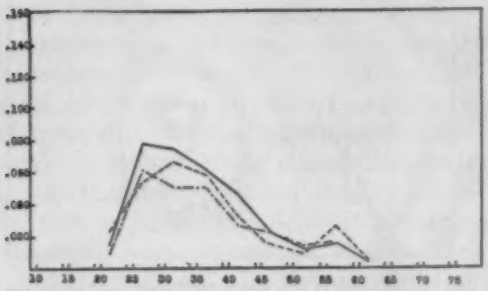

FIG. 2. CHRONOLOGICAL AGES AT WHICH THE MOST IMPORTANT CHEMICAL CONTRIBUTIONS WERE MADE, BASED UPON THE ESTIMATES OF THREE JUDGES. AVERAGE NUMBER OF CONTRIBUTIONS PER FIVE-YEAR INTERVAL

older men may therefore have disdained to publish findings which they regarded as of minor importance. If this explanation were a valid one the contributions of the older men should be superior in quality to the contributions of the young chemists. As a means of ascertaining the facts with reference to this matter, three of the writer's colleagues[5] from the chemistry department of Ohio University were asked to select the 100 *most important* or *most significant* chemistry contributions that were listed in Hilditch's book. The three collaborators were asked to make their selections without consulting one another and to make them from the point of view of the chemist rather than that of the layman. The peaks of the resultant curves appeared *at slightly younger ages* than the peak of Fig. 1 (see Fig. 2). Clearly, the alleged superior discriminative ability of the older men does not explain why maximum productivity occurs at such a relatively early age. Apparently, the most significant contributions to the science of chemistry have been made most frequently by individuals who were approximately thirty years of age at the time of making their invaluable contributions.

A chemist of more than national reputation was asked to explain why maximum productivity, as revealed by Fig. 1, is not sustained for a longer period of time. This chemist expressed the opinion that the early descent of the curve in Fig. 1 is due to the fact that the young chemist who displays marked ability by the early publication of important research findings is likely, sooner or later, to be advanced to an administrative position. That is to say, the more gifted young chemist is often made head of his department or placed in some other administrative post which absorbs his time and energy and precludes further research. The speaker added that it was his belief that the total amount of chemistry research might be greatly increased if the foregoing practice were to be abandoned.

If the present study had been limited solely to the creative work of chemists, one might accept the above hypothesis. However, age-curves similar to Fig. 1 were constructed for inventors, physicists, mathematicians, astronomers, poets, short-story writers and several other kinds of workers (see Figs. 3 to 9).[6] It is true that the precise age

[5] Professors Donald R. Clippinger, Frank B. Gullum and J. R. Morton gave cheerful and whole-hearted cooperation to this phase of the study.

[6] The data that were employed for the construction of figures 3 to 9 inclusive were obtained in a manner similar to that which was utilized for obtaining the data that were used for the construction of Figure 1. It is assumed that the reader will be more interested in the findings that were obtained than in a detailed description of the technique that was employed. Therefore most of the remainder of this manuscript will be devoted to a presentation of the findings.

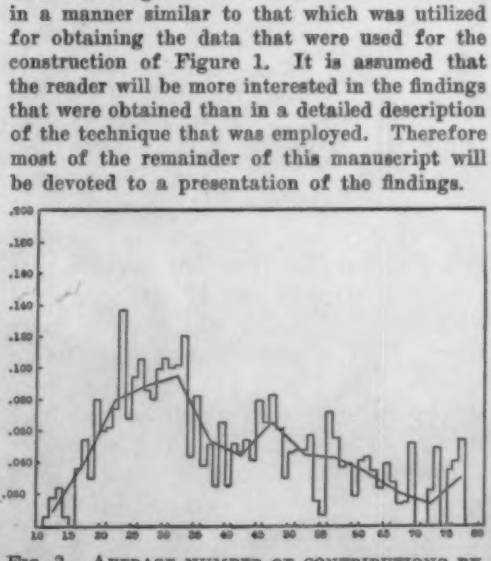

FIG. 3. AVERAGE NUMBER OF CONTRIBUTIONS BY MATHEMATICIANS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 453 CONTRIBUTIONS THAT WERE MADE BY 163 MATHEMATICIANS.

level at which the several curves reached their peaks varied somewhat. The shapes of the curves also differed. Nevertheless, for most kinds of endeavor that the writer has thus far studied, the peak has been found to appear at a relatively youthful age. Therefore, the theory that the early and marked decrement in the productivity of the chemists was due solely or even largely to the pressure of administrative and classroom duties is of very doubtful validity.

The data that are presented graphically in Fig. 3 were obtained from Cajori's "A History of Mathematics"[7] in the following manner. The author of this article first checked through Cajori's history and noted each mathematical contribution that was dated. By this means a total of 1,092 specific dates was obtained. A colleague[8] from the mathematics department was then requested to check from the writer's list those contributions which might be regarded as the most significant. After this had been done the list included 718 dates. Some of these were later discarded because they were duplicates; others were discarded because the birth or the death date of the contributor could not be found.

[7] F. Cajori, "A History of Mathematics," Second Edition. New York: The Macmillan Company, 1922.

[8] Thanks are expressed herewith to Professor Robert H. Marquis.

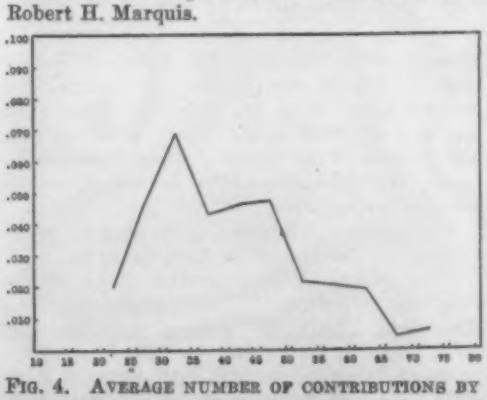

FIG. 4. AVERAGE NUMBER OF CONTRIBUTIONS BY PHYSICISTS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 141 CONTRIBUTIONS THAT WERE MADE BY 90 PHYSICISTS.

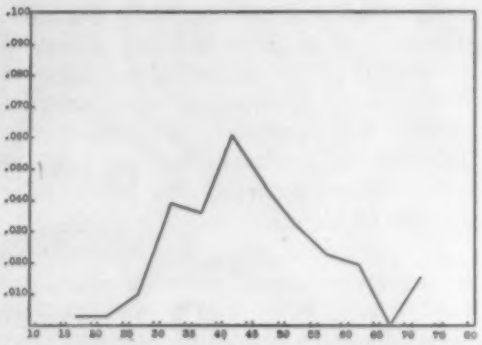

FIG. 5. AVERAGE NUMBER OF CONTRIBUTIONS BY ASTRONOMERS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 83 ASTRONOMICAL CONTRIBUTIONS THAT WERE MADE BY 63 ASTRONOMERS.

The data that were employed for the construction of Fig. 4 were obtained from William Francis Magie's "A Source Book in Physics."[9] This source book, which was first published in 1935, contains extracts from what Magie regards as the most important contributions that have been made to the science of physics during the past three centuries. It will be seen at a glance that Fig. 4 bears much similarity to the figures that have preceded it. Like the other curves this one rises much more rapidly than it descends and the period of maximum productivity is not later than from ages 30 to 34, inclusive.

The data that were employed for constructing Fig. 5 were obtained from "A Source Book in Astronomy"[10] by Shapley and Howarth. It differs from the preceding curves in that the peak of productivity occurs somewhat later, namely, between ages 40 to 44, inclusive. A part (possibly all) of this difference may be due to a difference in the amount of time lag that occurred between date of accomplishing and date of first publication. For 75 of the 83 astronomical contributions (90 per cent.) only the

[9] W. F. Magie, "A Source Book in Physics." McGraw-Hill Company, Inc., 1935.

[10] H. Shapley and H. E. Howarth, "A Source Book in Astronomy." New York: McGraw-Hill Book Company, Inc., 1929.

dates of first publication were available. Obviously these dates of first publication do not tell the chronological ages at which the contributions were actually made. We know only that for the 83 astronomical contributions the peak of productivity occurred *not later than* ages 40 to 44, inclusive.

If the present study had been limited to the creativity of scientists only, one might have been led to the spurious belief that classroom and administrative duties are the sole reasons why the foregoing curves start descending at such relatively early ages. A glance at the curve for great inventions should suffice to dispel such a belief. Fig. 6 presents the chronological ages at which 554 notable inventions were made by 402 well-known inventors. Since it sets forth *the average number of inventions per chronological age level,* this figure is comparable with the figures which precede it. The names of the inventors and their inventions were procured from sources some of which are listed in the footnote.[11]

When Fig. 6 was displayed to interested colleagues several commented, "What about Edison?" It is of course well known that Thomas A. Edison was very active as an inventor throughout his entire life. However, age 35 was Mr. Edison's most productive year. Moreover, during the four-year interval from ages 33 to 36 inclusive, Edison took out a total of 312 United States patents. This was more than a fourth (28 per cent.) of all the United States patents that were taken out by Edison during an inventive career that lasted for more than 60 years. Most of the data that were employed for the construction of Fig. 7 were obtained from Edison's official biography.[12]

Fig. 8 presents the average number of short stories per chronological age level per short story writer. This figure is based upon 1,396 short stories that were written by 220 writers. Data were obtained from Jessup's "Representative Modern Short Stories."[13] The dash line near the center of the figure presents data regarding the production of the 66 "best" short stories, which were written by 37 different authors. The "best" short stories were identified by examining books and lists that contained what were alleged to be the "best" short stories. Those stories which appeared most often in such lists were accepted as "the best." For the "best" short stories the age interval of maximum productivity is not later than ages 30 to 34, inclusive.

Fig. 9 presents data regarding *dates of composition* for 797 great poems. The sources from which the data were obtained (English, American and German) are listed in the footnote.[14] Since it presents not dates of first publication but dates of composition, this figure is not directly comparable with the graphs that have preceded it. The period of maxi-

[11] (a) "The Lincoln Library of Essential Information," The Frontier Press Company, 1934. (b) H. W. Ruoff, editor, "The Standard Dictionary of Facts." The Frontier Press Company, 1910. (c) E. W. Byrn, "The Progress of Invention in the Nineteenth Century." New York: Munn and Company, 1900. (d) "The Scientific American Reference Book." Compiled by A. A. Hopkins and A. R. Bond. Munn and Company, Publishers. Scientific American, 1905. (e) A. P. Usher, "A History of Mechanical Inventions," McGraw-Hill Book Company, Inc., 1929. (f) The *World* Almanac, 1935.

[12] F. L. Dyer, T. C. Martin and W. H. Meadowcroft, "Edison: His Life and Inventions." Harper and Brothers Publishers. In two volumes, vol. 2, pp. 975–1008, 1929. The writer is indebted to Mr. Henry Lanahan, general counsel of the Edison Company, for assembling and forwarding the necessary supplementary information.

[13] A. Jessup, "Representative Modern Short Stories." The Macmillan Company, 1929.

[14] (a) C. H. Page, "British Poets of the Nineteenth Century." New edition by Stith Thompson. Benj. H. Sanborn and Company, 1929. (b) C. H. Page, "The Chief American Poets." Houghton Mifflin Company, 1905. (c) B. J. Vos and P. A. Barba, "German Lyrics and Ballads: From Klopstock to Modern Times." Henry Holt and Company, 1923.

mum productivity for poetry seems to occur relatively early, namely, from ages 22 to 35, inclusive. Beyond age 35 the composition of poetry appears to decline rather markedly. The rise at the extreme right end of the curve is possibly significant, although it may also be accounted for in part by the fact that very few of the poets were alive at the age of 80. Average productivity at this advanced age may therefore not be trustworthy.

The reader will perhaps share the writer's skepticism that the early and marked decrement in the productivity of the chemists was due solely or even largely to the pressure of administrative and classroom duties when he learns that the age-curve (Fig. 6) for great inventions is very similar to the age curve for chemical contributions. Surely, it would be nonsensical to assert that the decrement in productivity among inventors was due also to the pressure of administrative and classroom duties. To what, then, was the decrement due? Were the older scientists incapable of producing at the same rate during their more mature years as they had produced when younger? And, if they were fully as capable of producing when older, why did they fail to continue their creative work? Some might suppose that, when the older scientist has won fame and professional recognition, the prestige-motive

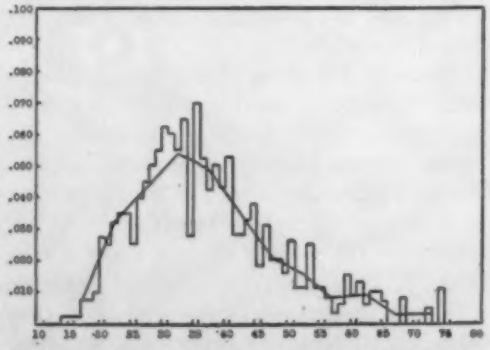

FIG. 6. AVERAGE NUMBER OF INVENTIONS BY INVENTORS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 554 INVENTIONS THAT WERE MADE BY 402 INVENTORS.

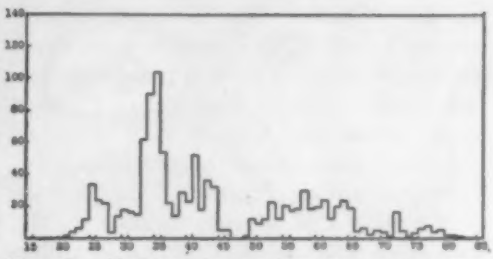

FIG. 7. NUMBER OF INVENTIONS THAT WERE MADE AT VARIOUS AGE LEVELS BY THOMAS A. EDISON. THIS GRAPH IS BASED ON A TOTAL OF 1,076 PATENTS.

that may have been eliciting strenuous and sustained effort might in some instances wane or even disappear. But would this be true also of the inventor and the short-story writer?

School texts usually picture the renowned scientist as a man of rather advanced years. The present findings suggest that in a number of fields the typical outstanding scientist is an individual who is in his late twenties or in his thirties at the time he is doing his most important creative work.[15] This fact is not a very important one so far as the scientist (or the layman) is concerned. The finding is one that will be of interest chiefly to the serious student of human behavior. The discrepancy, between the scientist as he actually is when he is at work and the stereotyped concept of the scientist, is probably due in part to the fact that personal fame spreads rather slowly. By the time the scientist has become an old man his fame is likely to be at its height and his photograph often taken. Under such circumstances the photograph may give rise to an erroneous impression.

It should not be assumed that Fig. 1 sets forth, for every type of work, the relative creativity of men of different chronological ages. The writer does not mean to convey the impression that

[15] Subsequent to the preparation of this manuscript the writer finds that in his "Dynamic Psychology," 1918, p. 130, Professor Robert S. Woodworth arrives at practically this same conclusion.

he has obtained typical creative-work curves. Indeed, he doubts very much that such a thing as a typical creative-work curve exists. This entire problem is of course one that is very complex. We need age-curves (or statistical distribution tables) for each kind of creative work. For, if one wishes to study the relative creativity of men of different chronological ages, the simple arithmetic average is insufficient. A simple hypothetical illustration will demonstrate the truth of this latter assertion.

Let us suppose, for example, that nine chemists have each made one contribution to their science. Let us assume also that one of the nine made his single contribution at age 80, that two of the others each made their contributions at age 50, and that the remaining six chemists each made contributions at age 30. In the foregoing hypothetical situation, the sum of the contributors' ages at the time of contributing is 360, and the average age at the time of making the contributions is $\frac{360}{9}$ or 40. Now, in spite of the fact that the average age of the contributors is 40, six of the nine contributions (two thirds of them) were made prior to age 40, namely, at age 30.

The situation is similar for the 993 chemistry contributions that are pictured in Fig. 1. For the 244 chemists, the average age at the time of making a contribution is slightly more than 38. But the five-year interval of maximum productivity is from ages 28 to 32, inclusive! This latter fact is one that would not be known if only the arithmetic average were available. Nor would the arithmetic average have revealed the fact that the curve in Fig. 1 rises so much more rapidly than it descends.

Inspection of the curve in Fig. 1 will help the reader to understand why this is true. If, in Fig. 1, the two sides of the curve were wholly regular and symmetrical, the average would yield a fairly accurate impression. For, in that case, one half of the contributions would have been made at ages younger than the average, and one half would have been made at ages older than the average. But in Fig. 1 the curve is not symmetrical. Because of this the average fails to reveal an adequate picture of the facts.

When inferences are to be drawn one must always guard against going beyond the factual data. For example, some might infer that Fig. 1 reveals the rise and the fall (by chronological ages) of the chemist's social and scientific usefulness. Such an inference would be wholly indefensible. The writer would remind such readers that, during their more mature years, the chemists that are included in this study may have been doing highly useful work of a kind that does not leave tangible results that can be dated. Thus, some of the older chemists may have been engaged in administrative work. And, even though this may not have been the chief reason for the diminution of their creative research work, the administrative work was very probably socially useful. Others of the older chemists may have been directing laboratories and teaching younger men the technique of chemistry research. It would of course be absurd to assert that the athlete could achieve equally good results without the counsel and advice of experienced athletic coaches. The situation is probably analogous for young chemists.

Some of the older chemists may therefore have been making their later contributions *through their students.* To some extent this hypothesis is undoubtedly valid. There probably is such a thing as "creative teaching." And, although its results do not show up in Fig. 1, this creative teaching may be fully as important to society as is that which is customarily referred to as scientific productivity. However, data for inventors, poets and certain other groups of workers should suffice to convince the

impartial reader that the foregoing hypothetical "reasons" do not explain why the curve in Fig. 1 wanes so noticeably beyond age 39.

One can hardly refrain from further philosophical speculation with reference to these findings. During the years that have passed, certain writers have assumed that the genius is one who is extremely capable, and some have assumed also that "genius will out." If this latter assertion be a valid one, why is it that the contributions of outstanding chemists (and certain other kinds of workers) decrease so rapidly during the workers' more mature years? Shall it be assumed that the world's most renowned chemists possessed *more genius* at ages 30 to 35 than was possessed by them at older chronological age levels? Certainly, such an assumption does not seem very plausible to the present writer.

Some have insisted that scientific discoveries are often very much like the finding of a five-dollar gold piece lying at the noon hour on the sidewalk at a busy street corner. Some one is bound to spy the coin. In other words, it is asserted that for years before it is seen, the discovery is "just waiting to be picked up." If this hypothesis be a valid one, why is it that the younger scientists find so many

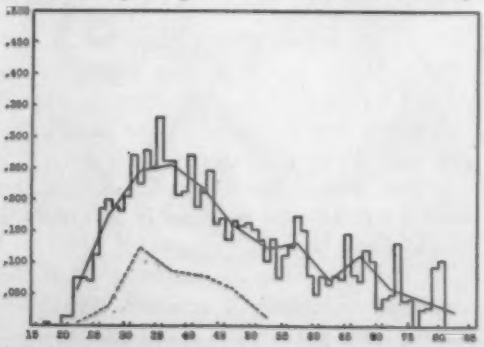

FIG. 8. AVERAGE NUMBER OF SHORT STORIES BY WRITERS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON 1,396 SHORT STORIES THAT WERE WRITTEN BY 220 INDIVIDUALS. THE DASH LINE PRESENTS INFORMATION REGARDING 66 "BEST" SHORT STORIES THAT WERE WRITTEN BY 37 DIFFERENT INDIVIDUALS.

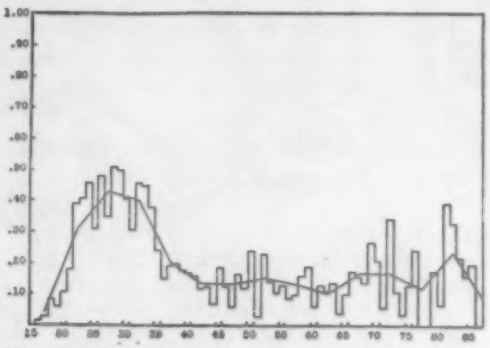

FIG. 9. AVERAGE NUMBER OF POEMS BY POETS DURING EACH YEAR OF THEIR LIVES. THIS GRAPH IS BASED ON A TOTAL OF 797 POEMS BY 82 POETS.

more of the "gold pieces" than do the older ones?

Assume for the moment that the alleged genius is merely the product of his environment, the instrument through which an idea or a creation receives expression. Why, then, are the thirty- and the thirty-five-year-old chemists, rather than the fifty- or the sixty-year-old chemists, so much more frequently the instruments through which the creation receives expression? Here certainly is a conundrum for the student of human behavior. The writer is not so rash as to pretend that he has entirely satisfactory answers to such queries as these.

In considering the results that have been set forth here, the reader should not overlook the fact that large individual differences exist.

It is certainly true that some individuals do their very best work when they are relatively old, and some do little that is really worth while until they *are* relatively aged. Therefore, if an individual has failed to make a chemistry contribution by the time he has reached the age of 35 or 40, it would be quite erroneous to assume that he will never make a contribution.

For the purpose of studying this the writer isolated data for 100 chemists, each of whom had made one contribution only that was dated by Hilditch. The data for these 100 chemists reveal

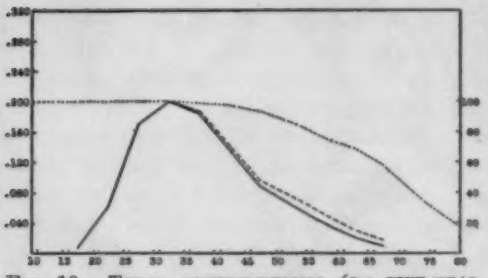

FIG. 10. TOTAL CONTRIBUTIONS (BY FIVE-YEAR INTERVALS) OF 244 CHEMISTS. ——— ACTUAL TOTAL; – – – – HYPOTHETICAL TOTAL; .... PERCENTAGE OF THE ORIGINAL NUMBER OF CHEMISTS THAT REMAINED ALIVE AT SUCCESSIVE AGE INTERVALS. FIGURES AT RIGHT REPRESENT PERCENTAGES.

that 34 per cent. of them made their first and only important contribution after they had passed age 40; 19 per cent. made their single contribution after they had passed age 50; and 5 per cent. did their first important research after they had passed age 55. One individual made his single noteworthy contribution at the age of 69! These individual differences suggest that it would be futile to attempt to ascertain the one chronological age level at which the chemist's social and scientific usefulness is at an end. Indeed, there probably is no such age level.

In Fig. 10 the solid line presents, by five-year intervals, the total number of contributions for the entire group of 244 chemists. This solid line in Fig. 10 made no allowance for the fact that more of the chemists were alive at some age levels than at others. In other words, Fig. 10 presents an undistorted picture of the actual number of contributions for each chronological age level. The dotted line in Fig. 10 presents the percentages of the original number of chemists that remained alive at successive age levels. This dotted line in Fig. 10 is so drawn as to be roughly comparable to the solid line in the following way. Had the living chemists contributed during their later years at the same rate as they were contributing from ages 30 to 34, inclusive, the solid line, which represents actual contributions, would have coincided with the dotted line which represents the percentages of the original number of chemists that remained alive at successive age levels.

The serious student of human behavior is, of course, not greatly interested in *what might have happened* under hypothetical conditions which did not actually exist. The psychologist is vastly more interested in *factual data*. Nevertheless, as a means of focusing attention upon the recorded facts, it may be worth while to speculate for a moment with reference to *what might have been*. Be it remembered, however, that this speculation is merely a device that is here employed for the purpose of presenting the true story of *what really happened*. With that fact firmly in mind let us proceed.

The dash-line in Fig. 10 is based upon the assumption that (had they lived) the deceased chemists of a given chronological age level would have contributed at about the same rate as did the group of living chemists of the same chronological age level. For example, at ages 65 to 69, inclusive, the deceased group was about 70 per cent. as large as the living group. It is here assumed that if this group of deceased chemists had remained alive they would have increased the contribution of the living group by about 70 per cent. Since, however, the contribution of the living group from ages 65 to 69, inclusive, was actually rather small, being only 10 contributions, a 70 per cent. increase would have raised the total by only 7 contributions—hardly as much as might have been anticipated.

If the foregoing assumption (that the deceased chemists of a given age level would have performed about like the living chemists of corresponding age level) is a valid one, the narrow band of space between the solid line and the dash-line in Fig. 10 represents that fractional decrement in scientific productivity which was due to the death of some of the chem-

ists. Fig. 10 thus enables us to make a rough hypothetical estimate of two things, namely, (1) the decrement in productivity which was due to death, and (2) the decrement which was due to factors other than death.

It will be noted at once that that decrement in scientific productivity which was due to death is relatively slight as compared with that much larger decrement which was due to factors other than death. If all the 244 noted chemists had lived until they were 80 years of age they probably would have made 57 more contributions than actually were made by them. This hypothetical increase amounts to slightly less than 6 per cent. of the actual total of 993 contributions. If those chemists who actually did live to a ripe old age had continued during their later years to contribute at the same rate as they were contributing from ages 30 to 34, inclusive, the total number of their contributions would have been 1,782 instead of 993. This would have amounted to an increase of 80 per cent. over and above the actual total of 993 contributions.

It was stated previously that the computation (80 per cent.) should not be taken too literally. We say this because society obviously can not lose that which it has never possessed. Moreover, the computation, 80 per cent., is based upon several unproven and unprovable assumptions. For example, this computation assumes that the number of possible chemistry contributions is practically unlimited; and that a given contribution does not depend upon antecedent contributions and cumulative social gains. Granted that the figure, 80 per cent., represents no actual social loss, the fact remains that our hypothetical computation demonstrates an actual and a very large decrement.

To those who regard scientific information as the most precious asset that the human race has accumulated, the above findings will perhaps bring a feeling of regret. Such persons may wonder if steps might not have been taken for increasing the scientific productivity of the older men. It is theoretically conceivable that an educational foundation, or some wealthy individual, might relieve the more capable research men of their routine and other time-consuming duties. If it be assumed that the older men have not lost their ability to do outstanding research work, the problem of actually motivating them (and certain other problems) would still remain unsolved. It is not always an easy matter to motivate a child—or even a white rat. How, then, would we proceed if we wished to motivate a Steinmetz? Or the poet who composes beautiful and immortal lyrics? (See Fig. 9.)

When he was thirty-five years of age Thomas A. Edison took out a total of 104 patents during the course of a single year. If he had maintained that same pace during the remainder of his life Edison would have given to the world approximately five times as many inventions as he actually contributed. Who would

TABLE I

AVERAGE NUMBER OF CONTRIBUTIONS PER FIVE-YEAR INTERVAL*

Type of contribution	CA Interval 10–14	15–19	20–24	25–29	30–34	35–39	40–44	45–49	50–54	55–59	60–64	65–69	70–74	75–79	80–84	85–89
Chemistry		.005	.052	.138	.165	.154	.115	.078	.063	.044	.026	.014	.022	.000	.004	
Mathematics		.011	.041	.081	.090	.096	.054	.045	.065	.045	.043	.034	.030	.014	.031	
Physics		.000	.020	.047	.070	.043	.046	.047	.021	.020	.020	.005	.006			
Astronomy		.003	.003	.010	.039	.036	.061	.045	.033	.023	.020	.000	.016	.000	.000	.048
Invention		.006	.031	.043	.054	.052	.036	.021	.017	.009	.010	.003	.003	.000	.003	
Short-story writing		.001	.057	.175	.248	.257	.218	.160	.122	.130	.070	.106	.059	.037	.021	
Great poetry	.002	.041	.310	.434	.404	.197	.136	.133	.153	.136	.102	.168	.161	.117	.234	.058

* The peak of each statistical distribution is underlined.

TABLE II
SUMMARY OF FINDINGS WITH REFERENCE TO CREATIVITY

Type of endeavor	No. of workers	No. of works	Median age	Mean age	Standard deviation	Years of maximum productivity
Chemistry ..	244	993	35.95	38.08	10.45	28–32
Mathematics.	163	453	37.88	41.12	14.10	34–38
Physics	90	141	38.00	39.34	11.05	30–34
Astronomy .	63	83	44.20	45.09	12.00	43–47
Invention ..	402	554	35.27	36.71	11.10	31–35
Short-story writing ..	220	1,396	38.17	40.75	11.90	33–37
Great poetry	82	797	32.86	38.18	15.75	26–30

criticize Edison unfavorably because he failed to do this? And who would cast aspersions on the world's most gifted chemists because of the decline in their productivity? Certainly, the present writer does not mean to imply that the scientist exists solely for the purpose of making scientific contributions. Indeed, this study makes no assumption whatsoever as to what ought to be. Nor is this investigation concerned primarily with the problem of *ability* to do creative work. On the contrary, we are here dealing only with *performance* — the specific kinds of performance that have been set forth.

Upon the basis of the foregoing data, what is one to conclude? Do the decrements in creativity at the older age levels imply a corresponding decrement in ability to create? Certainly not! It seems probable that outstanding potential ability must be present in the individual who makes original and noteworthy contributions in any field. However, potential ability alone does not guarantee accomplishment. Indeed, it is doubtful that genius is solely the fruit of any single trait. It is the belief of the writer that the fruits of genius are, on the contrary, a function of numerous integers, including both the personal traits of the individual worker, environmental conditions that are not too hostile, and the fortunate combination of both personal traits and external conditions. If the foregoing hypotheses be valid ones, the following conclusion seems inescapable. At the older chronological age levels, some one or more of the numerous variables that are essential to the fruition of genius tend either to wane or to disappear. At the present time we are not able to identify all the factors that enable a particular individual to exhibit genius. Therefore, in instances where the fruits of genius are lacking we are not able to state precisely which of the needed ingredients are wanting. As regards the decrement that has been set forth herein, it can be said only that the decrement is probably due not to any single causal factor but rather to a complex group of undetermined causal factors. Any statement much more specific than the foregoing would obviously be highly speculative and of doubtful validity.

NOTE: Subsequent to the preparation of the above article the writer has constructed age curves for more than forty kinds of endeavor. These curves serve to confirm the writer's belief that generalizations regarding the creative years need to be made with extreme caution. For example, it has been found that literary masterpieces of the first rank have been produced most frequently by authors who were from ages 40 to 44 inclusive. This statement applies to the publication of books that are placed most often on "best books" lists. However, when the various types of literature are considered separately the peaks of productivity are found to vary rather widely.

GALILEO AND SCIENTIFIC HISTORY

By Professor LANE COOPER

CORNELL UNIVERSITY

QUITE unexpectedly to me, some heat appears to have been aroused by a little book of mine called "Aristotle, Galileo, and the Tower of Pisa," a volume published last year by an emergent University Press of Cornell University. The object of the book was to present the evidence concerning the alleged experiment or experiments of Galileo about the year 1590 from the tower of Pisa in public demonstration of an alleged mistake by Aristotle with regard to falling bodies—namely, the supposed misstatement by him that, in falling, heavier bodies will move faster in proportion to their weight. The evidence, so far as it appears in languages other than English, is presented in translation and also in the original tongues; and, in particular, the requisite passages from works of Aristotle, passages which have hitherto not been confronted with the evidence from Galileo's writings, or *vice versa*, are made accessible to the kind of reader I had in mind—the teacher, say, of mathematics, physics or the like, or the reader of "scientific history" who has no ready access to the ancient and the modern foreign languages or at all events none to the original sources of this tale concerning an ancient Aristotle and a modern Galileo.

The passages I thought desirable to translate from the Italian of Galileo's first "Dialogue Concerning Two New Sciences" (1637) and the Latin of his earlier treatises "De Motu" (about 1590) are duly recorded and opened for the reader in my book. But in *Nature* (London), January 4, 1936, pp. 8–9, Professor A. S. Eve calls attention to passages from Galileo's dialogue which might well have been quoted in my volume. I should gladly consider including some part of the added quotations if another edition were called for, but must now remind the reader, first, that the passage on falling stones in the dialogue of 1637 echoes a much earlier passage of "De Motu" (see my page 50) in which Galileo about the year 1590 ironically talks of what would happen if two spheres of lead were let go from the moon; and, next, that the "Sagredo" of the dialogue, who says he has "made the test" with falling bodies and who represents views entertained by Galileo, is not to be identified with him. Galileo is referred to in the dialogue as an authority outside of it, "our Academician" (*cf.* the Edizione Nazionale of Galileo's Works, Volume 8, page 54). "Sagredo" bears the name of an actual person, a friend of Galileo. He is a Venetian. Mr. Eve says, "It is certainly disconcerting to find Sagredo altering the height from 100 to 200 cubits," that is, from the 100 cubits of "Salviati," the other interlocutor who holds similar views on the fall of bodies from a height; we may note that whereas the tower of Pisa is 178 feet high, the well-known tower at Venice is, or was, 323. If we are to go behind the imaginative dialogue in search of historical fact, should we not argue that the historical Sagredo at Venice, and not Galileo at Pisa, had in this instance "made the test"? It is better not to press the point; we may yet find the utterances of "Salviati" and "Sagredo" more vulnerable than some have thought. Contrary to the statement of "Salviati," Aristotle does not say that "an iron ball of one hundred pounds falling from the height of one hundred cubits reaches the ground before a one-pound ball has fallen a single cubit." He does not use that kind of

language at all. He does not say fall or ball or one hundred or cubits or anything of the sort; he states general laws of motion, using the Greek words to express the concepts of magnitude, shape, weight and proportion, and the letters of the Greek alphabet as we use A, B, C, D, rather than the quantities of arithmetic; and it is important to study what he does say before supplying any of his generalizations with an example. "Salviati" goes on: "I say they arrive at the same time." That is not true, as he immediately shows: "You find, on making the experiment, that the larger outstrips the smaller by two finger-breadths, that is, when the larger has reached the ground, the other is short of it by two finger-breadths; now you would not hide behind these two fingers the ninety-nine cubits of Aristotle."

Behind those two fingers there are said to lurk laws, about the movement of heavier bodies through viscous media, out of which the qualified student of physics can make some defense of Aristotle, considering his time and circumstances, in his statements about lightness and weight, media and motion. Aristotle says that the heavier body moves faster over an equal space than the lighter; that they move differently in proportion to their magnitudes or sizes and the density of the medium. As he expresses the proportion, it does not necessarily mean either ten to one, or a hundred to one, though it has been taken to mean both. Can it be both? After all, "Salviati's" *two fingers* and "Sagredo's" *span* express a proportion. And what about the ratio of 120 to 395 which is noted at the end of the present article?

Aristotle says that a moving object cleaves the medium either by its shape or by the impulse which the body that is carried along or is projected possesses; the assertion sounds reasonable in these days of Big Berthas and streamlined cars and airplanes. He says that the downward motion of a mass of gold or lead or of any other body endowed with weight is quicker in proportion to its size, a statement to which we shall return, for a diving airplane is clearly a body endowed with weight. He says that would not be true in a void; the Greek word is commonly taken to be the equivalent of our "vacuum"; it surely implies the absence of any medium to retard the moving body. Professor E. K. Rand tells me that Aristotle's reasoning on the conduct of objects of different weights in a vacuum brought about a change in the atomistic theory: "After his remarks it was no longer possible to imagine that objects of different weights falling in parallel lines in a void would ever catch up with one another." Aristotle's utterances as they have just been outlined are in support of his contention that the void, a perfect vacuum, is impossible. They have been assailed as if they were main, and not subsidiary, positions with him. They are supplied with chapter and verse in my book; and I hope that some master in physics will study them there or in Aristotle's works, not with respect to the tower of Pisa, but, let us say, with respect to the fall of a dummy man from mid-air out of a diving airplane. Since Aristotle does talk in "De Caelo" of letting something go from mid-air, let us say it might mean from a height of 18,000 feet.

As for Mr. Eve's citation to show that Galileo knew what he was criticizing, there is ample evidence that Galileo read Aristotle. On this head, I resisted the temptation rather to quote extensively from the early treatise "De Motu," which belongs to Galileo's interval of teaching at Pisa, and reveals his study of Aristotle, anticipating his later attacks, and also to go into the question whether he consulted the "Physics" and "De Caelo" in Greek. Viviani says that Galileo took up the study of Greek in youth. Since he wrote Latin, however, Galileo doubtless often followed a pro-

cedure not without a parallel in our day, and, for economy of time, mainly referred to the works of Aristotle in quite available Latin translations.

A harsher critic, the learned Aldo Mieli, in a recent number of *Archeion*, pages 303–7,[2] takes my book to task for an ignorance of physics which it freely admits. Since I am supposed to be a student of language and literature, my personal interest lay in tracing the growth of a myth from a posthumous story about "repeated experiments" said to have been made in public by Galileo as a young teacher at Pisa, yet never referred to by him or any one else in all the controversies of his career; its growth, that is, into the now widely accepted tale that by a single dramatic experiment he worked a revolution in physical science. No such miraculous change has been brought about in one day in any age by Galileo or another. Signor Mieli is perhaps unduly severe through a misunderstanding of some parts of the book which were hopefully meant to be humorous, but have fallen without prosperity on a Continental ear. But, apart from that, in Italian eyes of to-day it no doubt was a mistake to call a German, Wohlwill, the best authority on Galileo's experiments at Pisa, or to join with him and Hugo Dingler in finding something odd about a passage where Galileo, at the approximate date, 1590, of the alleged experiments, does speak of his observation of falling bodies; where in fact he says he has "often tested this"—namely, that, in free fall, *wood starts off faster than lead!* It seems to have been a queer traditional notion that certainly did not disappear as the result of "experiment" or "experience" by Galileo. The Latin word *experientia* embraces both concepts. Can the passage on wood and lead rightly be used in support of the tale about the alleged experiments from the leaning tower?

Favaro so uses it in an article on the credibility of Viviani's Life (our source for the story of Galileo and the tower), in *Archivio storico italiano*, anno 73 (1915), Volume 1, pages 340–3, where he attacks the position of Wohlwill. Bending to the criticism of Mieli, I cheerfully direct attention to this article, but must call attention also to Favaro's article, not mentioned by Mieli, upon "some inexactitudes" of Viviani, *ibid.*, anno 74 (1916), Volume 2, pages 127–50. Much as we respect the learning and devotion of Favaro, chief editor of the great National Edition of Galileo, we still recommend the scepticism of Wohlwill and Dingler on the point at issue. The credibility in the main of Viviani's Life does not depend upon our accepting one tale with an inherent improbability—that Galileo *repeatedly* made experiments from the tower in the presence of *all* the teachers and students at his university.

If reputable authors of our generation accept this tale one from another, repeat it with absurd variations, take words from a dialogue of Galileo for the words of Aristotle and never look into Aristotle to see what he says and in what connection, how are we to think of the critical standards of Viviani in the year 1654? More than a century later, the competent William Bartram, American botanist and zoologist, can thus describe one party in an alligator-fight ("Travels," 1794, page 116): "His plaited tail brandished high, floats upon the lake. The water like a cataract descends from his opening jaws. The earth trembles with his thunder." Was his tail brandished high, or did it float? Aristotle in his "Poetics" notes how a story grows. He says that people like to add interesting details in the belief that it will gratify the listeners. Lives of Galileo and histories of science bear him out.

[2] The pages, not the whole number, have reached my publisher.

The end of the present article may likewise illustrate that impeachment.

Mr. Eve remarks: "No doubt Lane Cooper is correct in saying that Galileo was flogging a dead horse, and that many had already attacked the rash statement of Aristotle." And the passage from "De Caelo" about "the downward movement of a mass of gold or lead, or of any other body endowed with weight," that it "is quicker in proportion to its size," he calls "very suggestive of [Aristotle's] erroneous view of falling bodies." Now my book tells of persons both before and after 1590 who are said to have made the like test, though I was not bent on mentioning any after 1606 save Coresio and Renieri; Eiffel is said to have experimented with all sorts of objects from his tower 984.25 feet high. I did not talk of "a dead horse," nor call any statement of Aristotle's "rash," nor think that what he says about the downward movement even of gold, to say nothing of *any other body endowed with weight,* would be erroneous if the medium through which it moved were dust and not air. But in my book I did go too far in holding that it is quite erroneous with respect to air, or such is my present notion in my very imperfect knowledge of physics. When writing my book, I believed with Galileo, Gregory, Hart and many another, that if you went to the top of the leaning tower and dropped a ten-pound weight and a one-pound weight, or a hundred-pound weight and a one-pound weight, when they reached the ground together, or nearly so, it would prove Aristotle wrong with respect to the downward movement of bodies through air. I went along with the experts who would let a teacher of English believe that it made no difference whether the proportion between the bodies was ten to one or one hundred to one, whether the tower was 100 cubits or 200 cubits high, whether the bodies hit the ground simultaneously or with a slight interval in the times of alighting and whether that interval was two fingers or a span. I do recall being troubled by a discrepancy between Aristotle's use of algebraic symbols in "De Caelo" 3.2 and the subsequent reduction of them to arabic numbers. Now I wish to leave the question in the most capable hands, and refer all readers to the section entitled "Aristotle's Dynamics," pages 26–33, in the admirable edition of Aristotle's "Physics" by W. D. Ross, Oxford, 1936, who says (page 29):

> In fact, having observed accurately that motion through a denser medium is slower than through a rarer one, he makes the natural enough mistake of supposing that velocity and density *ceteris paribus* vary inversely; failing to notice that the relation which connected them might be more complex than that of inverse proportion. A better mathematician might, even in the absence of evidence, have noticed the possibility of this.

The same writer mentions the correspondence of Hardcastle and Greenhill in *Nature* (92: 584–5, 1913–14; 93: 428, 1914) on the terminal velocity of projectiles, and on the bearing of that problem upon the statements of Aristotle and Galileo. These letters came to my attention after I had virtually finished the present writing; so also an unverified reference to motion-pictures of raindrops, with the tempo of the pictures slowed so as to show how the larger and heavier drops pass the smaller and lighter in mid-air.

Meanwhile are they who, in the face of weak evidence, still suppose that a theatrical Galileo might well have ascended the tower, after due advertisement, to perform modern experiments with free fall before the mob—are they really honoring that great and good man? If Stokes and Rayleigh had lived in Galileo's day, would either have lent himself to a display of the sort? As a person necessarily unversed in the latest physical research, I yet humbly ask what significance should attach to an episode like that? Since publishing my book, I have heard of something mentioned above,

namely, laws respecting the movement of heavier and lighter bodies through viscous media, one of which laws apparently could be tested for "the downward movement of a mass of gold or lead, or of any other body endowed with weight," through air, somewhat better if you took the objects up in an airplane and dropped them from a point several miles high than if you dropped them from any celebrated tower in Italy. For me, so far as I can understand the law, it goes better with the statements of Aristotle than with the story of Galileo; better, for example, with the passage about "a bit of earth let loose in mid-air"—"the more there is of it the faster it moves." Of course it is fun to feel wiser than Aristotle, though the fun commonly entails a loss of historical perspective. It is also fun to go faster than anybody could in the days of Galileo. Herewith we reach our proportion of 120 to 395, and our terminal velocity.

Our experimenter shall now be the late Mr. Collins, who wrote for *The Saturday Evening Post* (Philadelphia) of February 9, 1935, and knew about free fall from personal experience. His article, here quoted by editorial permission, is entitled "Return to Earth, or Diving Ten Thousand Feet Straight Down." We read (page 51):

> I did five speed dives first. These were to demonstrate that the ship would dive to terminal velocity. Contrary to popular opinion, a falling object will not go faster and faster and faster and faster. It will go faster and faster only up to a certain point. That point is reached when the object creates by its own passage through the air resistance to that passage to equal in pounds the weight of the object. When that point is reached, the object will not fall any faster, no matter how much longer it falls. It is said to be at terminal velocity. A diving airplane is only a falling object, but it is a highly streamlined one, and therefore capable of a very high terminal velocity. A man falling through the air cannot attain a speed greater than about a hundred and twenty miles an hour. But the terminal velocity of an airplane is a lot more than that. . . .
>
> I went to eighteen thousand feet for the final one. . . . I eased the throttle back, rolled the ship over in a half roll, and stuck her down. I felt the dead, still drop of the first part of the dive. I saw the air-speed needle race around its dial, heard the roaring of the motor mounting and the whistle of the wires rising, and felt the increasing stress and stiffness of the gathering speed. I saw the altimeter winding up—winding down, rather! Down to twelve thousand feet now. Eleven and a half. Eleven. I saw the air-speed needle slowing down its racing on its second lap around the dial. I heard the roaring motor whining now and the whistling wires screaming, and felt the awful racking of the terrific speed. I glanced at the air-speed needle. It was barely creeping around the dial. It was almost once and a half around, and was just passing the three-eighty mark. I glanced at the altimeter. It was really winding up now! The sensitive needle was going around and around. The other needle read ten thousand, nine and a half, nine. I looked at the air-speed needle. It was standing still. It read three ninety-five. You could feel it was terminal velocity. You could feel the lack of acceleration. You could hear it too. You could hear the motor at a peak whine, holding it. You could hear the wires at a peak scream, holding it. I checked the altimeter. Eight and a half. At eight I would pull out.

IRRATIONAL RHYTHMS

By Dr. JAMES B. SHAW

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FROM the earliest times of history number has fascinated man, and he has endowed it with mystical properties. Three, five, seven, ten and other numbers have had potencies which man has tried to use. Even to-day numerology has its devotees, and among them many who are supposed to be enlightened, and not all the devotees of mystical number are aware of their religion. How number came into the consciousness of man no one knows. It is quite possible it was forced into his consciousness by the rhythms of his daily life. The pulsing of his heart after a long chase, the rhythms of his steady walk home, the beat of waves on the shore, the succession of day and night, the arrangement of the petals of flowers, the phyllotaxis of leaves on a stem—these and many others would inevitably be noticed and from them would come the idea of the integer and the ratio. After he learned how to add, subtract, multiply and divide he would have noticed that his result would always be within his list of numbers. After the invention of negative numbers he would not be hampered in his subtractions, and now he would have a complete field of number, the rational numbers, which would possess an endless system of rhythms. Every prime number would start a new set of ripples in the currents of his intelligence. In the course of time the classification of integers and ratios was completed. Thousands of theorems have been found for them, and occasionally even to-day some one prints a new set of remarkable relations for certain numbers.

However, as early as the time of Pythagoras it was known that there were numbers not in this list. If we lay off the side of a square on the diagonal, and then the remainder of the diagonal on the side, and so on, we will never find a small segment left over which will measure those before exactly. It is easy to see this is the case, for if we lay off the side on the diagonal and then draw from this point a perpendicular to the diagonal to meet the side, it will cut off the proper segment, and we then have again a side and a diagonal of a square to compare. That is, after two steps we are just where we started. A phyllotaxis like this would go around and around, but never would the terminal point be just over the initial point. Pythagoras knew of other irrationals, and they wrecked his philosophy, for he thought the universe could be expressed in terms of ratios and integers. Even to-day there are those who say the universe is simply the incarnation of mathematics.

In the flow of the centuries numerous irrational numbers were found, and as the study progressed they became more and more complicated. A wilderness of number sprang up, and there seemed to be no basis for classification of them which was not artificial. However, early in the last century, there appeared in France a turbulent young genius named Évariste Galois. He had a stormy career, since he mixed up in the politics of the day. He was a great annoyance to his instructors, for he had too many original ideas, which they did not understand, and they were afraid of him. It is said that one day he heard of geometry. He at once went to the library, found the treatise of Legendre, read it through in the afternoon and threw it down with the remark that it was so simple there was nothing to it. He died in 1832 at the

age of scant twenty-one, from a shot in a duel. The duel was evidently a frame-up of his enemies. The night before, he sat up all night writing out what he had found out about algebra, and left a thin memoir for a friend to publish. This has been the subject of much study ever since, and has been the spring from which have come many deep investigations. One of the important things in it is the basis for classifying irrational numbers that come from algebraic equations, and Galois also discovered the essential rhythms they possess, so that the wilderness has turned out to be really a landscape of wonderful designs. We will look at a simple example of what Galois was talking about, in order to understand what he said.

Suppose we study the irrational number which may be written in the form:

$$R = \sqrt{(2+\sqrt{2})(3+\sqrt{3})}.$$

This is an answer to the equation

$$x^8 = 24x^6 - 144x^4 + 288x^2 - 144.$$

There are also seven other answers as follows: S, which is found by changing in R the sign in front of $\sqrt{2}$; T, which is found by changing the sign in front of $\sqrt{3}$; and U by changing both these signs. Then the other four are $V = -R$, $W = -S$, $Y = -T$, and $Z = -U$. We shall see that there are very remarkable properties connecting these eight numbers. In the first place, we can find that S is expressible in terms of R as a polynomial, thus:

$$24S = -R^7 + 20R^5 - 60R^3 - 24R.$$

Likewise we have

$$12T = R^5 - 18R^3 + 36R$$

and

$$12U = R^7 - 22R^5 + 102R^3 - 120R.$$

Then the other four would be found from these by merely changing all signs. This is very remarkable, because usually if we solve an equation and find all the answers, we can not express them as polynomials in a single one. But, what is still more remarkable, if we start with S in place of R, or with T, or with U, these same polynomial forms, written out in terms of S or of T or of U, will also give the entire set of numbers. In other words, these polynomial forms have the property that whichever number we start with, they will furnish all the numbers. In such a situation we call the set of eight numbers *conjugates*. To go further, if we write out the polynomial form for S in terms of S instead of R, and reduce the result, we would be surprised to find that the final result is $-R$. That is,

$$24R = S^7 - 20S^5 + 60S^3 + 24S.$$

The same will be true for each of the other forms; that is, if we change the one for T by writing T instead of R, and reducing, we shall find in the end that it is also $-R$. The form for U written out in terms of U instead of R will also give, when reduced, $-R$.

But we are not done with the problem. Suppose we write the form for S in terms of T instead of R, what will be the result? The answer is that when reduced it gives U. On the other hand, if we write S for R in the form for T, it reduces to $-U$. And now we see what else to look for. If we write U in place of R in the form for T we shall find upon reducing it that we have S. If we change this about and put T in U for R we shall have $-S$. Finally, if we put S for R in the form for U it reduces to T, and if we put U for R in the form for S it reduces to $-T$. This possibility of writing in any one of the four letters is certainly something we might not expect at first. We see that putting in the other four will merely change all signs, for there are only odd powers of R in the forms, and the other four letters are the negative numbers for the first four. Since the result of starting with R, constructing any polynomial and then substituting in it

some other letter for R, comes out one of the eight letters, and is therefore a polynomial in R, we evidently have a closed set of processes. The performance of any one and then writing in the result of some other gives us one of the processes. We will see this better perhaps if we write out the things we have so far found out in a small table of the effects of the eight processes. For simplicity we will call the construction of the polynomial which gives S, by a single letter I. Also the process which gives T we will call J, and that which gives U will be K. Then we will have to have a label for the statement R = R, and this will be E. We will also have to write for the process giving V, $-E$; for that giving W, $-I$; for that giving Y, $-J$; and for that giving Z, $-K$. Then we have the table:

E	I	J	K
I	$-E$	K	$-J$
J	$-K$	$-E$	I
K	J	$-I$	$-E$

This means, for instance, that the effect of doing the process J after we have done I will be $-K$, or J after K will be I. That is, if we first get S then put it in the polynomial for T we will have $-U$ or Z. Since this table gives also the products of the numbers used in quaternions called 1, i, j, k we call it a quaternion table. We say that the polynomials above form a quaternion group. And we call the original equation a quaternion equation, and the numbers R, S, ... Z, we say belong to a quaternion field. We mean by that to say that the numbers arising from adding, subtracting, multiplying or dividing these eight numbers will always be a polynomial in R and therefore equally also a polynomial in either of the eight. In short: the rational numbers compounded with any one of the eight numbers R, S, ... Z, will always give a number which can be written rationally in terms of R or S or T, etc. Moreover, if we construct any polynomial whatever in terms of R, there will be similar polynomials in terms of S, T, etc., and the numbers they represent will be a set of conjugates, either eight different numbers or four different or two different or all the same number.

We are not done, however. For if we go back to R we see in it $\sqrt{2}$, and $\sqrt{3}$, and we might inquire: Are they expressible in terms of R and then in terms of any other of the eight letters? The answer is: Yes. We find indeed that

$$24\sqrt{2} = -R^7 + 20R^5 - 60R^3.$$

Also

$$12\sqrt{3} = R^6 - 21R^4 + 84R^2 - 72.$$

We may further put in

$$24\sqrt{6} = -R^7 + 24R^5 - 132R^3 + 144.$$

This means now that any combination of rational numbers, these square roots, and any of the eight numbers may be reduced to a polynomial in terms of R, and therefore in terms of S or the others.

We are now in a position to say that as far as rational numbers and the three square roots above are concerned, all their combinations by the four processes of addition, subtraction, multiplication and division, and their combinations with the eight numbers which are the quaternion conjugates never arrive at anything but rational numbers and powers of R, or of S, $\cdots$. That is, in brief, R defines a Galois field of the quaternion type, every number in it being a polynomial in R. An equation like the one we started with is called a *normal equation* and the field a *normal field*. What Galois said amounted to the proposition that all irrational numbers arising from algebraic equations are made up of numbers belonging to normal fields. The entire list of algebraic irrationals, then, is classified into the types coming from the normal fields. The first problem is to find out what are the normal fields and what kind of equations are normal equations. For instance, there are but five

types of fields, each of which has eight conjugates. The quaternion type we have just studied is one of the five. The normal equations for the other four are not complicated. For a field with a prime number of conjugates there is but one type, the cyclic. That means that if we form the polynomial from R for S, and then keep substituting in it the new conjugate, we shall come back at last to a form which reduces to R. For an example suppose

$$R^3 = 21R - 35,$$

then we will have

$$S = R^2 + 2R - 14,\ T = S^2 + 2S - 14,$$
$$R = T^2 + 2T - 14.$$

For each type of normal field there will be a set of processes similar to I, J, K, $\cdots$ which when combined will give all the conjugates. A table of their combinations can be made out as we did above. Such a table gives us the results of combining the various processes that make up the Galois group belonging to the field. For each normal field there is a group. When we study the groups by themselves we find that for a given number of processes N, there is possible only a finite number of groups, but we do not yet know whether in every case there is a field for any given group. For the possible groups of order 8, we do have normal equations, however, and we would expect any group to have a field belonging to it. If the group is studied as a problem in combinations we call it an abstract group. For every abstract group the corresponding Galois groups will be endless, but all with the same structure. We find then an underlying design in the order that gives us the various combinations. And since all the conjugates may be considered as the various numbers belonging to a single entity we have unity in the structure of a normal field. In fact, the single number is the one we call the hypernumber which is the complete root of the equation. We can not enlarge on this.

There is another feature which we will notice, since it explains something further which Galois said. If we arrange the terms of the equation above properly we may write it in the form

$$(x^4 - 12x^2 + 12)^3 = 6(2x^3)^2.$$

This enables us to see that if we use not only rational numbers but the $\sqrt{6}$, then we can take a square root of each side and the result is an equation of the fourth degree. It may be solved as a quadratic giving R in a square root form which has integers and $\sqrt{6}$. In this form we may construct U in terms of R and we will find the polynomial form is cyclic of order 4. So also we might write the equation in the form

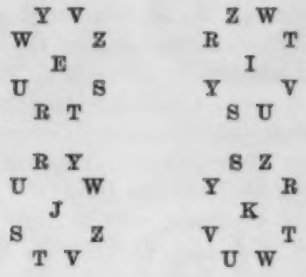
$$(x^4 - 12x^2 + 36)^3 = 2(6x^2 - 24)^2.$$

If we are allowed to use $\sqrt{2}$, this also reduces to a fourth degree equation from which we can find R, and then S would be given by a polynomial which is again cyclic. And if we write the equation in the form

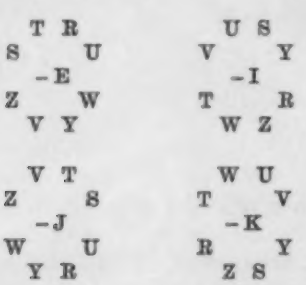
$$(x^4 - 12x^2 + 24)^3 = 3(4x^2 - 12)^2$$

we could arrive at a cyclic quartic by using $\sqrt{3}$. So we see what Galois meant when he said that if we could use rational numbers and another number properly chosen from an irrational field, we would be able to reduce the degree of the equation.

We will now see what happens when we perform the eight processes in turn upon the entire set of eight conjugates. We can see this best in a series of diagrams. We will use E to represent the original diagram, then I the one produced by changing each letter according to what the polynomial I gives, likewise for J, and K, and we will write these four in the center of the figures. We have then the four diagrams:



The reader will find it interesting to discover the simple rule for shifting the letters. As a hint they change in pairs, always the same way. The effect of the processes – E, – I, – J, – K will be shown in the following diagrams:



The reader will find that each of the eight processes when repeated will ultimately bring back the original diagram. E of course will always give it. – E has to be used twice. The others four times around will come back to E. These are eight positions of eight dancers, which in the various shifts could be called the Quaternion Dance. There is a similar diagram for every normal field. We could also look upon these diagrams as representing flowers with the petals of different colors. A flower would belong to the field only if the petals were arranged in one of the possible ways. In the case above the petals would have eight colors, and just eight possible arrangements, if it bloomed in the Galois quaternion field.

What Galois saw and reported was this new type of beauty: the beauty of the irrational. It is remarkable that the vast array of algebraic irrationals can be reduced to polynomials in terms of any selected primitive number from each of the different Galois number fields, and that the number of possible fields is after all not so very large, if we do not consider equations of too high a degree. The list of normal equations is equally not so very large. Whatever algebraic equation we have to solve, the problem amounts to finding what normal equations we need in order to have the necessary fields from which come the irrationals in the final expression of x. For instance, to solve a quintic may require square roots and then numbers from a complicated field of order 60, called an *icosahedron field*. There are 60 conjugates, none expressible in radicals, but from any one all of them may be found rationally. Will there ever be a Galois for the irrationals like π which are not algebraic? The study of automorphic functions is a step in the right direction, and in series of orthogonal functions we make progress. So we may predict there will be another Galois for these irrationals. Let us hope for him a life which is not tragic.

SOLUTIONS UNDER HIGH PRESSURE

By Dr. R. E. GIBSON

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If I take some table salt and mix it with water, the salt, as you all know, will disappear and I shall be left with something that looks like water but is not nearly so pleasant to drink—in other words, a salt solution. You have all performed this experiment and may have wondered what became of the salt. For several centuries chemists have exercised their minds over this question, but they have not found a satisfactory answer. In the process, however, they have thrown considerable light on the nature of matter and incidentally of our own mechanisms—we shall not, however, dwell further on this point.

Factors Affecting Solubility

You know, furthermore, that if I take enough salt I shall reach a state when no more will disappear or dissolve and the salt solution will be "saturated." The amount of salt which goes into the saturated solution is called its solubility. Different substances have different solubilities in water or other liquids. You also know that the solubility of a substance changes with temperature. If, to make an eye lotion, we dissolve boric acid in boiling water we know that, although the solution is perfectly clear when hot, it will deposit a mass of crystals of boric acid after it has cooled. The solubility of boric acid in hot water is much greater than in cold water.

What perhaps you do not know is that if I take a saturated solution of a substance, keep its temperature constant and squeeze it with a very high pressure I shall be able to make the solution either deposit crystals or dissolve more. Generally speaking, the former process will take place if I squeeze the solution hard enough. It is to this change of solubility with pressure that I wish to call your attention, telling you briefly how the subject is studied, what advances have been made and what consequences of general interest the work may have.

Solubility in Relation to Pressure

Prior to 1931 very little was known about changes in solubility under pressure. A few workers had made good observations, but the number of solutions studied was small and the pressures used comparatively low, not exceeding 1,500 atmospheres or bars, *i.e.*, about 22,500 lb./sq. inch.

In 1931 Dr. L. H. Adams of the Geophysical Laboratory published the results of a systematic study of the solubility of common salt (sodium chloride) in water up to pressures of 16,000 atmospheres (240,000 lb./sq. inch). These were the first experiments of their kind ever made and were followed by investigations of the solubility of potassium sulfate, ammonium nitrate and potassium iodide over the same wide range of pressures. I shall not enlarge upon the details of these researches but will merely emphasize that each of these four substances behaved in a seemingly highly individualistic way as regards its change of solubility with pressure.

As will be seen in Table I, no features common to the behavior of all the four salts were immediately obvious. This was somewhat disappointing for, as I shall discuss later on, one of the objects of this work was to discover some general

TABLE I

SOLUBILITY OF DIFFERENT SOLIDS IN 100 GRAMS OF WATER AT VARIOUS PRESSURES AND 25° C.

Substance	Weight of solid dissolved in 100 grams water				
	Pressure: Atmospheric	1,000 bars	4,000 bars	7,000 bars	10,000 bars
Sodium chloride .	35.9	36.2	38.3	37.8	36.9
Potassium sulfate	12.1	16.0	19.5	18.2	16.0
Ammonium nitrate	208.9	161.0	89.6	59.5	42.3
Potassium iodide.	148.0	152.0	161.0	172.0	181.0

laws that might hold for changes in solubility in all solutions under pressure. Further study showed, however, that general features common to the behavior of the solutions of all these and of other salts were indeed present, but they lay below the surface and had to be dug out.

How was this done? In the first place, I should say that although it is possible to determine directly how much of a given solid water or any other liquid dissolves under very high pressures, it is not always convenient or necessary to do so. The well-established science of thermodynamics shows us that in order to determine changes in solubility with pressure the only measurements we have to make under pressure are those of volume changes—a relatively simple thing to do. Let us, therefore, consider for a moment how the volumes of solutions change under pressure.

METHOD OF PROCEDURE

If we take, say, water, put it into a strong cylinder and force a piston into the cylinder we shall generate a very high pressure and at the same time the water will shrink a small amount. This small shrinkage is called the *compression* of the water if the original volume is one cubic centimeter. If now, to the water we add some solid, say common salt, we shall find that, for a given pressure, the compression of the solution is less than that of the pure water. The difference is very small but highly significant, and the measurements must therefore be made with great precision. The stronger the solution the less does its compression for the same rise of pressure become.

These small differences between the compressions of pure water and those of solutions of different concentrations up to the saturated solutions are all the measurements we need under pressure, and, when these are combined with quantities which are measured at ordinary pressure, we obtain the solubility of the substance in water at different pressures. You will see, then, that if we find any law about how the volumes of solutions change with pressure we may immediately extend it to a slightly more complicated law about how the solubility of the dissolved solid changes with pressure.

LAW GOVERNING CHANGES IN VOLUME WITH PRESSURE

For solutions in water we found such a law—it had been proposed over 40 years ago by Gustav Tammann—a brilliant idea which had been tested with the fragmentary data then available but not developed. This hypothesis suggested that water mixed with a solid (in a solution) behaves like water that is under a pressure greater than the external pressure, as if the salt pulled the water molecules closer together than they normally are.

When this simple hypothesis is developed we find that, if we know the compressions of water over the whole pressure range from 0 to 12,000 atmospheres (180,000 lb./sq. inch), and, if we determine from measurements at low pressures the internal pressure that the salt appears to exert on the water, we may calculate the compressions of any of the solutions over this whole range of pres-

sure. When the results of these calculations are compared with experimental observations we find excellent agreement.

To summarize, therefore, we may say that it is in general possible to calculate the compressions of solutions of solids in water up to the highest pressures under which the solutions will exist simply from observations at low pressures. I should emphasize, however, that had it not been for the experiments on volume and solubility changes in solutions made at the Geophysical Laboratory we should still be in doubt as to the validity of Tammann's hypothesis and any conclusions based thereon would be subject to suspicion.

Returning to the main subject of this article, namely, the influence of high pressure on the solubility of a solid, we may state that this influence is in general larger than was previously expected and that in the case of solutions in water it may be predicted by the method I have outlined from considerations which involve only measurements at low pressures. I have sought to extend the results to solutions in solvents other than water and have found the same conclusions to be true, but this work is still in progress.

While the results I have just outlined are of some interest in themselves, I must confess that their main importance lies in their being one step towards the elucidation of larger problems. They have a direct bearing on the problem of the nature of liquids and on the forces of attraction and repulsion which exist between the molecules of solids and liquids, an important subject which unfortunately can not be elaborated in this short article.

The Problem of Rock Formation

This work was undertaken primarily to provide a physicochemical basis from which to attack the problem of rock formation under pressure. Let us return momentarily to the boric acid solution. We started with a hot, clear solution: it cooled to room temperature, became supersaturated and deposited solid boric acid. If we had cooled it further the water itself would have frozen and we should have been left with a solid mass of boric acid and ice.

It is now generally accepted that the earth was once a liquid like the boric acid solution, only much hotter and infinitely more complex. As it cooled, it deposited complicated masses of crystals until practically all was solid, giving the rocks with which we are all familiar. In principle, then, the formation of rocks resembled the crystallizing of the boric acid solution, with this difference that, whereas the pressure in the boric acid solution was small, the pressures miles down inside the earth where rocks either crystallized or whither they sank immediately after solidifying are enormous, being measured in tens of thousands of atmospheres. If, then, we are to understand the way in which rocks were formed we must know not only the solubility of solid minerals in molten silicates at high temperatures, but also how this solubility is influenced by high pressures. It is possible to examine the behavior of minerals under high temperatures and pressures—indeed, such work is being done at the Geophysical Laboratory and elsewhere—but the technique is extremely difficult and very slow.

Modern scientific theories of matter give encouragement to the belief that, if we find regularities in the behavior under high pressures of simple solutions that can be examined at room temperature, the results may be modified and applied with success to other solutions at higher temperatures. With this in mind we are attempting to prepare a background of knowledge of the behavior of solutions under pressure in the way I

have described to you, but, while I believe that this background will prove of fundamental importance in solving some of the problems of rock formation, I do not want to give you the impression that this application of the results is immediately possible. Much more work will have to be done.

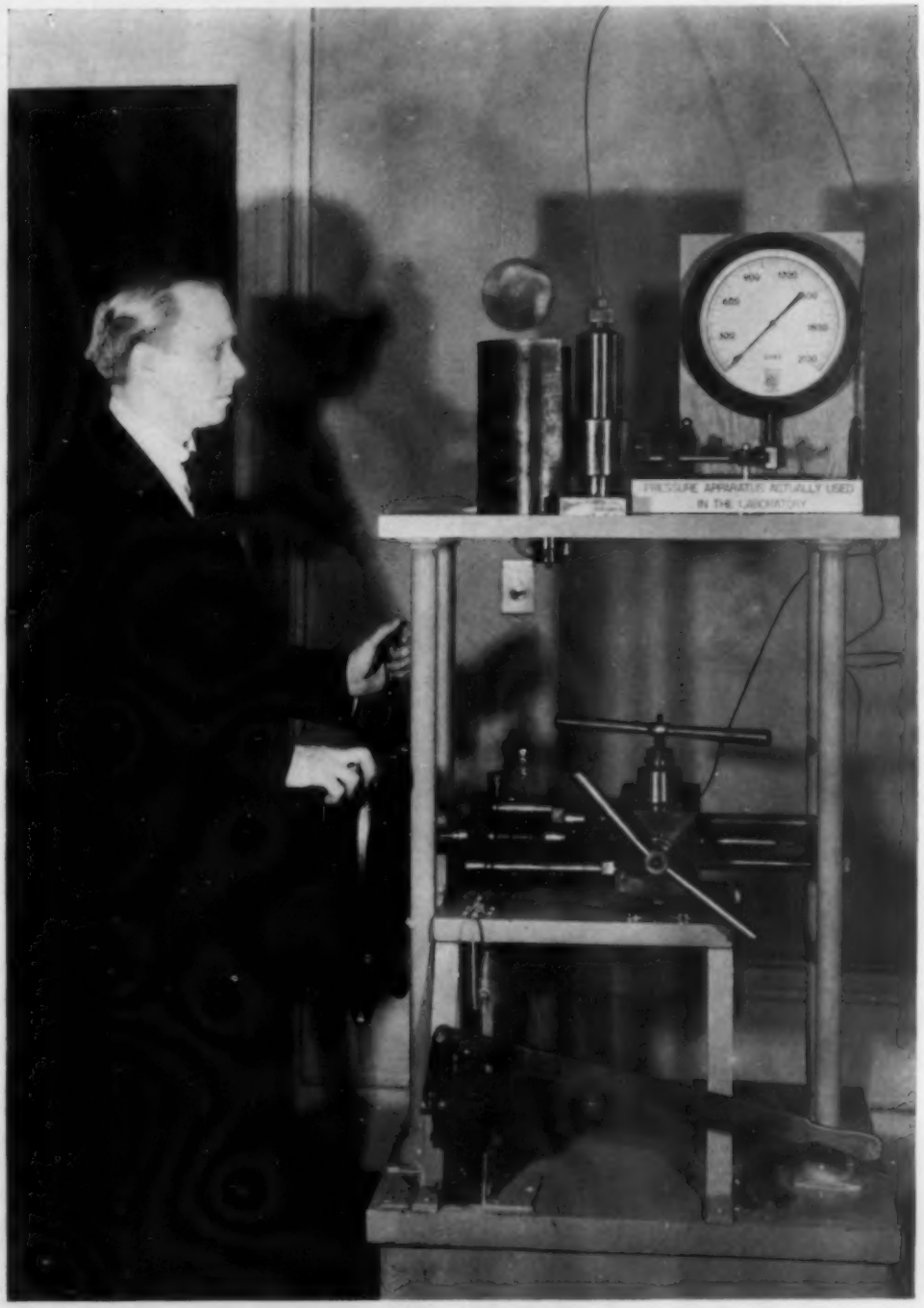
The Temple of Science

The advance of our understanding of nature may be likened to the building of a magnificent edifice, such as a cathedral; we are building an intangible temple of organized knowledge. All the facts, like the building stones, must be quarried from nature with much labor; they must be hewn and polished, sculptured and fitted together to form the various members of the building. By the genius of some master mind or architect, these members are gathered into a beautiful and stable structure, which not only delights the mind but withstands the onslaught of time.

The building of this temple of science proceeds steadily, but only occasionally does its progress catch the eye or appeal to the popular imagination. A new or startling type of fact wrested from stubborn nature arouses our interest, we catch to some extent its importance, above all it arouses our curiosity. When a tower or some outstanding feature of the building is completed, when some all-embracing theory such as the theory of relativity links together many members of the structure, the world at large looks on with interest and amazement. Between the new types of facts and the finished tower there is a large gap. The tower rests on foundations hidden from the average person. If the structure is to stand the test of centuries, these foundations must be systematically and carefully laid, each stone must be carefully examined, hewn and fitted accurately into place. This part of the work of building is what distinguishes modern science from ancient philosophic speculation.

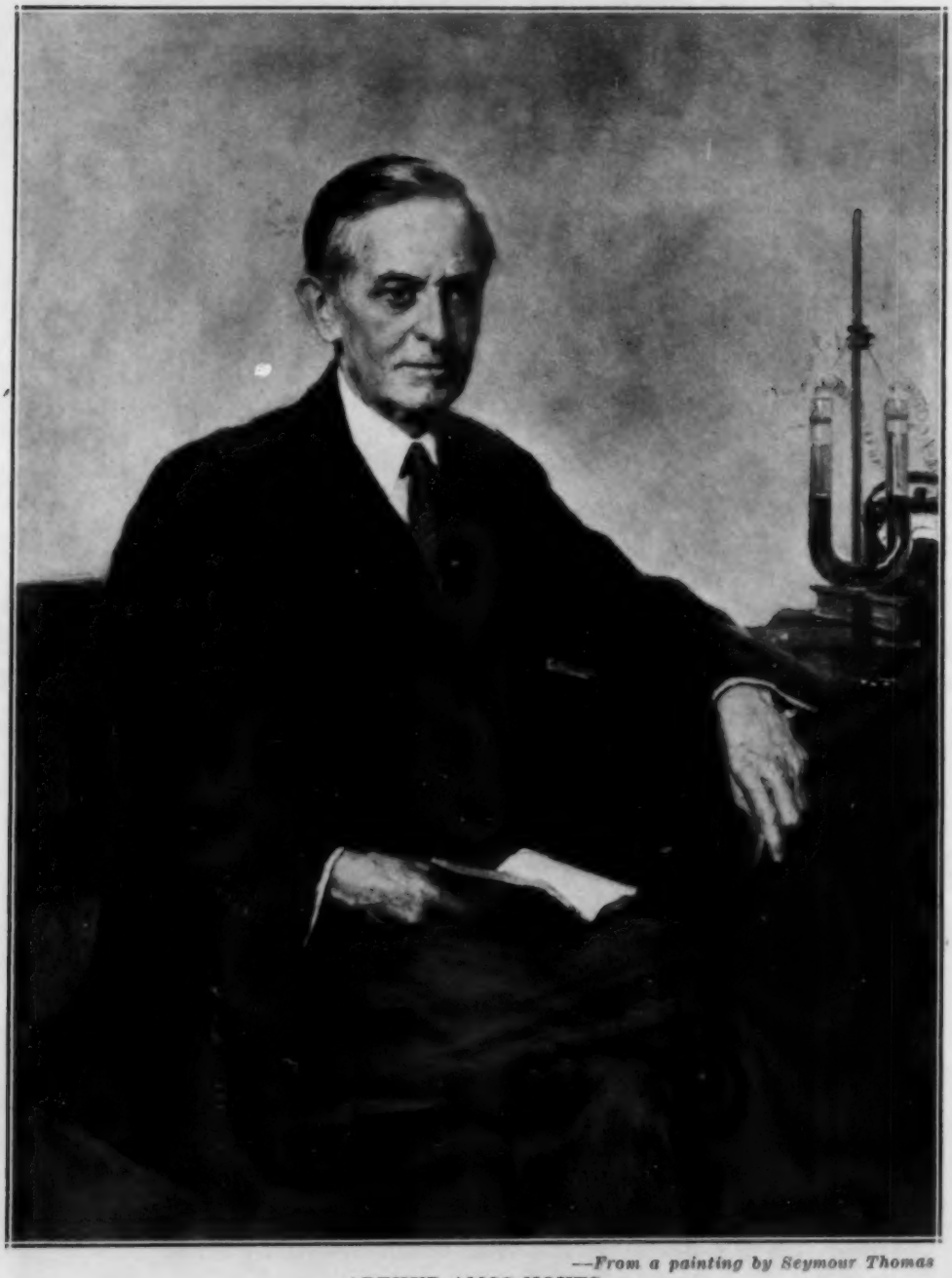
In the finished temple of science the work I have just described may form one of the courses of blocks deeply hidden in the foundations of a tower or buttress. I have tried to show you how the stones were quarried, polished and fitted together.

And why should we work to build this non-material temple of organized knowledge, why did Andrew Carnegie, whose one-hundredth birthday we have just celebrated, give so liberally of his substance towards its advancement? Surely it is to the end that ignorance and superstition at least may not impede us, our children and the generations yet unborn from living richer, freer and grander lives.



THE PRESSURE APPARATUS

WHICH ENABLES THE INVESTIGATOR TO RECORD ACCURATELY THE BEHAVIOR OF SOLUTIONS UNDER PRESSURES THAT ARE COMPARABLE TO THOSE IN NATURE WHEN ROCKS ARE FORMED.



—From a painting by Seymour Thomas

ARTHUR AMOS NOYES

THE PROGRESS OF SCIENCE

ARTHUR AMOS NOYES, AN APPRECIATION

THE death of Arthur Amos Noyes, which occurred at Pasadena on Wednesday, June 3, removes from the scene one of the outstanding figures of American chemistry during the past half century. His scientific career covered the entire period from 1886 to 1936. An evaluation of Noyes's contribution to American chemistry can be made only in the light of the history of chemical science in America during that time. When A. A. Noyes was graduated from the Massachusetts Institute of Technology in 1886, chemical research in America was almost unknown. It is true that Remsen's Laboratory at the Johns Hopkins University had then just recently been founded and an opportunity was there afforded Americans to acquaint themselves with research methods in chemistry, an opportunity of which a number of eminent American chemists took advantage. But, by and large, chemical research in America was confined to a very few individuals who struggled on under highly disadvantageous conditions and whose work attracted but little attention among their contemporaries. American universities at that time did not consider that the fostering of research was one of their primary purposes.

Although Noyes's early contact with chemistry lay in the fields of analytical and organic chemistry, fields, moreover, which he continued to cultivate—particularly, the former—throughout the rest of his life, he was innately a physical chemist. Although he was not especially trained in physics and mathematics, he had a natural physical sense which enabled him to analyze problems from the physical point of view, to devise experiments to answer crucially important questions and to carry out these experiments with a precision which went beyond that of other experimenters engaged in the same fields. Coming under the influence of Ostwald, at Leipzig, he returned to America to found the new subject of physical chemistry, although for a number of years he continued to teach organic chemistry at the Massachusetts Institute of Technology.

It is not possible to enumerate here the many important contributions that A. A. Noyes made to physical chemistry. It is illuminating, however, to point out that one of his earliest contributions was the enunciation of what has come to be known as the "Solubility Product Principle." Prior to that time, it was supposed that in a solution of an electrolyte the concentration of undissociated molecules remained fixed so long as the compound remained present as solid phase. Noyes recognized that, if this were true, it must also be true that the product of the concentrations of the ions under the same conditions must remain constant. The solubility product principle proved much more useful than the principle of the constancy of concentration of the unionized molecules and we now know that the solubility product principle is, in fact, true, while the alternative principle seems to have no foundation in fact, since the concept of a neutral molecule has no meaning in the case of solutions of electrolytes or, indeed, in the case of solid electrolytes.

The application of the solubility product principle to the problems of analytical chemistry led to most important results, and Noyes was among the foremost in applying these principles to analytical problems. Indeed, throughout his life, Noyes actively cultivated the field of analytical chemistry and his monographs written with Sherrill and with Bray are authoritative at the present time.

During the latter years of the nineteenth century and the early years of the present century, the problem of electrolytes was one of the foremost engaging the attention of physical chemists. In spite of the success of the ionic theory in many directions, its failure in the case of strong electrolytes was recognized at an early date. Noyes was foremost among the many investigators who bent their energies to the elucidation of this important problem. While the ultimate solution was not reached by Noyes or any other physical chemist, nevertheless, the exact data which Noyes and his co-workers collected on equilibria, solubilities, conductances, freezing points and transference numbers of electrolytes paved the way for the final solution, which was provided only a dozen years ago by Debye and Hückel and Onsager. In the experimental field, Noyes went beyond the workers of his day. He was particularly active in investigating the properties of aqueous solutions of electrolytes at high temperatures, and the work turned out under his direction in the Research Laboratory of Physical Chemistry at the Massachusetts Institute of Technology remains practically the only available source of information in this important field.

Greatly as Noyes was devoted to research, he was equally outstanding as a teacher. His lectures, whenever and wherever given, were models. It was due to Noyes more than to any other one man that the method of teaching physical chemistry in America was revolutionized. He constantly emphasized the importance of general physical principles and illustrated their use and application by the problem method. To-day, practically all texts of physical chemistry follow the method introduced by A. A. Noyes.

If Noyes was outstanding as an investigator and as a teacher, he exerted a still more profound influence upon American science as a research director. Noyes early recognized the need of training prospective chemists in the way of research, and he recognized, in addition, that such training could be afforded only in a properly equipped laboratory. Through his own exertions, he founded, at the Massachusetts Institute of Technology, in 1903, the Research Laboratory of Physical Chemistry which has served as a model of research laboratories throughout the United States, if not throughout the world. Here, for the first time in America, an opportunity was afforded young investigators to carry out their researches in a well-equipped laboratory and under conditions that were ideal. In no other laboratory in the country was it possible at that time for young men to engage in their own researches without the need of eking out a living by undergraduate instruction or by other means. Noyes introduced the idea of post-doctoral fellowships. They were not known as such then but were called research associateships. At the research laboratory, Noyes also introduced research assistantships for young men who had not already attained the doctorate.

The research laboratory was not a place where young men came to get direction, but rather a place where they came to carry on research. They came not merely to work under the immediate guidance of any one individual, but, when they were adequately equipped and had adequate research programs, they were afforded an opportunity to carry on their own researches in an independent fashion. The success of the research laboratory under the direction of A. A. Noyes is attested by the large number of eminent chemists who once were associated with that laboratory; such as G. N. Lewis, W. C. Bray, R. C. Tolman, W. D. Harkins, F. G. Keyes, E. W. Washburn, John Johnston, to mention only a few. The old roster of the research laboratory now reads almost like

a membership list of the National Academy of Sciences.

Arthur Amos Noyes was an ideal director. While he kept in touch with the work of men engaged on his own problems, he, nevertheless, gave them complete independence in carrying out such work. In the case of others, who were working on their own problems, Noyes always exhibited helpful interest, but never interfered with the individual investigator; every worker in the laboratory not only had an opportunity to solve his own problems, but it was expected of him to do so. It was this impersonal point of view with regard to the problems in the research laboratory that contributed perhaps more than any other factor to the success of the laboratory. Yet Noyes was always interested in the men themselves and was ready to help them on every occasion.

After 1915, Noyes became associated with the Throop Polytechnic Institute, which, through his efforts in conjunction with those of George E. Hale, was ultimately converted into the present California Institute of Technology. Here again, Noyes showed his outstanding ability as a scientific organizer. No one who has watched the development of the California Institute of Technology or of the Gates Chemical Laboratory of that institution can fail to see the influence of Noyes's keen insight. Noyes leaves behind him at the Massachusetts Institute of Technology and at the California Institute of Technology two living monuments to his imperishable fame as founder of American physical chemistry.

Although Noyes spent much of his energy in organizing the research laboratory at the Massachusetts Institute of Technology, he, nevertheless, was always active in research. While many of these researches were carried out with the aid of collaborators, Noyes himself constantly supplied ideas and invariably was the most severe critic in the analysis of the final results. His devotion to research is well illustrated by the fact that during the last two years of his life, when suffering from the aftermath of several painful operations, he continued his work on "Argentic Salts in Acid Solution," the results of which investigation were published in three papers appearing in the 1935 issues of the *Journal of the American Chemical Society*.

Active to the last, A. A. Noyes leaves behind him a heritage of rich accomplishment; his works live after him to do him honor and to benefit mankind.

CHARLES A. KRAUS

THE AWARD OF THE FRANKLIN MEDALS TO DR. KETTERING AND DR. JEWETT

THE Franklin Medals were presented at the annual Medal Day exercises of the Franklin Institute in Philadelphia on the afternoon of Wednesday, May 20. This year the recipients were two Americans who have greatly influenced the lives of their fellowmen by their genius for invention: Dr. Charles Franklin Kettering, of the General Motors Company, and Dr. Frank Baldwin Jewett, of the Bell Telephone Laboratories.

The award to Dr. Kettering was based upon "his significant and timely contributions to the science of automotive engineering—a science out of which has grown the greatest industry in this country, the manufactured product of which has, in only a quarter of a century, changed the face of the civilized world."

Among the inventions listed as the reason for the award may be mentioned his first, a small electric motor powerful enough to rotate automatically the mechanism of a cash register. His next important contribution, which changed and increased the usefulness of the automobile, was his idea of an electric self-starter for automobiles. The automobile

DR. CHARLES FRANKLIN KETTERING

companies of that period (1911–1912) were not at all enthusiastic about his proposals, but an accident to the close friend of the president of one of these companies, caused by cranking a car, gave Kettering the opportunity to demonstrate how cranking could be eliminated. In 1912 his invention was awarded the Dewar Trophy by the Royal Automobile Club of London for the greatest advance by any motor car maker during the year.

Other outstanding inventions and developments include "Delco-Light," an electric power plant especially designed for farm and rural residences; Frigidaire, of which company Mr. Kettering is vice-president; and several others developed under the name of the General Motors Research Corporation, of which Mr. Kettering is vice-president and director. In the report upon which the award is based, his research which resulted in "Ethyl gas" is also mentioned.

The presentation of a Franklin Medal to Dr. Frank Baldwin Jewett was given "in recognition of his many important contributions to the art of telephony, which have made conversation possible not only from coast to coast, but from

DR. FRANK B. JEWETT

this country to the other side of the world—contributions of which some were made by him alone and some by him in collaboration with other workers in the great laboratory of research which he organized and which he has directed with such signal success.''

Dr. Jewett began his work with the American Telegraph and Telephone Company in 1904. At this time the telephone field was on the threshold of a great expansion and the value of scientific research was just beginning to be appreciated. Dr. Jewett was called upon to organize a research laboratory to aid in the solution of the many problems involved in the extension of long distance lines from coast to coast in the United States, and in 1915 this was accomplished. This phenomenal extension of commercial telephone service was accomplished by the introduction of phantom loading and the loading of large-gauge and open wire circuits, among other things. By the end of 1915 it was possible to transmit speech across the Atlantic Ocean. It was twelve years, however, before commercial telephone service was opened between England and the United States. These achievements were the direct out-

come of the scientific work carried on in the laboratory under Dr. Jewett.

The Franklin Medal, the highest award for physics in the United States, was founded in 1914 by Samuel Insull, of Chicago. It is ordinarily awarded to one outstanding American and a scientist from some foreign country. This year, however, the committee recommended the awards to the two Americans mentioned above.

Medal Day was instituted by the late Dr. Howard McClenahan in 1926, and has become one of the important events in the recognition of accomplishments in the field of science. It brings together in Philadelphia internationally known scientists and diplomats and thus pays tribute to the workers in the laboratories as well as the engineers of great administrative ability.

The Franklin Medal is awarded "to those workers in physical science or technology, without regard to country, whose efforts, in the opinion of the Institute, acting through its Committee on Science and the Arts, have done most to advance a knowledge of physical science or its application."

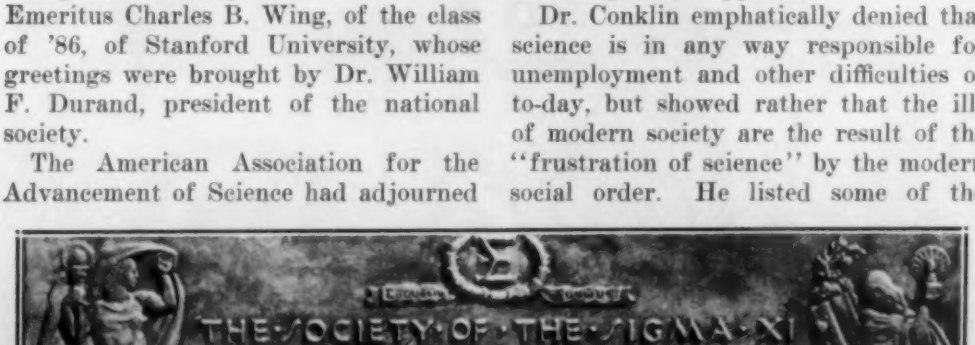
E. D. C.

THE SIGMA XI SEMI-CENTENNIAL

COMMEMORATING the founding at Cornell University fifty years ago of the Society of the Sigma Xi, nearly seven hundred scientists, research workers and guests from all over the country came to Ithaca on June 19 and 20. Included among them were Dr. Frank Van Vleck, of Washington, D. C., William A. Mosscrop, of Hempstead, N. Y., and John Knickerbacker, of Troy, N. Y., three of the men who, while at Cornell University, founded the organization. The only living founder not present was Professor Emeritus Charles B. Wing, of the class of '86, of Stanford University, whose greetings were brought by Dr. William F. Durand, president of the national society.

The American Association for the Advancement of Science had adjourned its meeting in Rochester earlier in the week to Ithaca. From the first address by its president, Professor Edwin G. Conklin, of Princeton University, following President Farrand's welcome, the tenor of the meetings was the importance of scientific research to social progress. Again and again, eminent speakers pointed out that, great as has been the progress of science in the past, the very foundations of civilization depend upon the continuance of research, unhampered and adequately supported.

Dr. Conklin emphatically denied that science is in any way responsible for unemployment and other difficulties of to-day, but showed rather that the ills of modern society are the result of the "frustration of science" by the modern social order. He listed some of the


THE SOCIETY OF THE SIGMA XI
DEVOTED TO RESEARCH IN SCIENCE
HAS PLACED THIS TABLET HERE
ON ITS FIFTIETH ANNIVERSARY
TO COMMEMORATE THE FOUNDING
AT CORNELL UNIVERSITY
1886 1936

THE SIGMA XI MEMORIAL TABLET

DR. HENRY S. WILLIAMS
FIRST PRESIDENT OF THE FIRST CHAPTER OF SIGMA XI

startling contrasts between the status of knowledge and present conditions, to show graphically the "conflict between progress and stagnation, knowledge and conduct, science and the social order."

Dr. Durand, responding to the addresses of welcome, recalled that in the last twenty-five years Sigma Xi has grown from a constituency of 7,500, largely within the United States, to 35,000, engaged in scientific research in fifty-five countries.

Dr. Van Vleck, recalling the inception of the society, gave credit to two classicists, whose names had not before been mentioned, for an important part in the founding of Sigma Xi. Van Vleck and Wing, technologists, were often consoled, he said, by their two room-mates, George L. Burr and Charles H. Thurber, because, "soaked in science" as they were, the two engineers could not be elected to Phi Beta Kappa. In reply, both Wing and Van Vleck predicted that a new society for scientific men would arise. Such a one was definitely suggested to him, he said, by Day as the two toiled up the hill from town in the spring of 1886. Dr. Van Vleck also revealed, for the first time, that it was Burr who first suggested using Greek letters for the name of the new society and who evolved its Greek motto, later approved by Professor Henry Shaler Williams, first president of

THREE FOUNDERS OF SIGMA XI WITH THE PRESENT PRESIDENT
LEFT TO RIGHT: WILLIAM A. MOSSCROP, JOHN KNICKERBACKER, DR. FRANK VAN VLECK AND DR. WILLIAM F. DURAND

the society. Van Vleck and the other two founders present were honored at a reception in Willard Straight Hall on Friday afternoon.

In the great Memorial Room adjoining, throughout the two days, was set up a research exposition of twenty-five striking demonstrations of some of the research being carried on at Cornell University by the younger members of Sigma Xi. Just inside the door were displayed in glass cases some of the original records and other items of historical interest of the society.

Dr. Karl T. Compton, president of the Massachusetts Institute of Technology, speaking on "The Service of Sigma Xi in the Universities of the Future," pointed out that "the geographic pioneer is now supplanted by the scientific pioneer, whose thrill of discovery or urge for reward is no less keen and whose fields of exploration are probably unlimited. Without the scientific pioneer, our civilization would stand still and our spirit would stagnate; with him, mankind will continue to work toward his higher destiny." From his experience of two and a half years as chairman of the Science Advisory Board, set up by the President and disbanded last December, the speaker described the scientific work carried on by the government and reiterated a recommendation made by the board to the President and not yet acted upon, for the establishment of a permanent scientific advisory council for the coordination and general supervision of the government's scientific work in non-military departments. Speaking of

science in industry, he showed that those industries which have liberally supported research have been financially more prosperous than those which have not and advocated that technically trained men be given an increasing share in the industry's management. Educational institutions, Dr. Compton said, offer the greatest opportunity for the search for truth:

> Scientific discoveries are the seeds of industry and public welfare, and the universities are the nurseries in which they are produced and nurtured to the point where some of them can be transplanted into the fields of industry. I once likened new industries to babies: they need shelter and nourishment, which they take in the form of patent protection and financing. But, before all, they need to be born, and their parents are science and invention. Neither laws nor committees, nor juggling acts, nor wishful thinking can perform the first necessary step of conception. To maintain and advance our civilization we need more and better scientific seeds, and industrial babies. The educational institutions of higher learning are the birthplaces of this new knowledge, as well as the training and proving grounds for the young men and women who will carry this knowledge on and put it to practical use.

The speaker urged that opportunity for science depends greatly upon "overcoming the instinctive prejudice of the scientist against having his work publicized."

Dr. Max Mason, president of the Rockefeller Foundation, speaking on "Science and the Rational Animal," traced the spirit of learning through the three centuries since it came into being. "Man's real problem is himself," he said, citing the disturbances in physical and mental health which have come with civilization and how they are met with knowledge. Advocating the objective approach to the art of living, devoid of personal prejudice, Dr. Mason invited membership in a new organization, Alpha Omega, dedicated to the scientific way of living; its password, "How do you know it?" followed by the question, "What of it?"

"The Alpha Omegas are not universally popular," he said, "for they take an aggressive attitude toward some of the foibles of their friends. They do this because they believe that little things add together to make large things, and that mental attitudes are contagious. . . . They do not keep their passwords secret; they use them every day. . . .

"We can not be true to the spirit of science in our laboratories and false to

THE MEMORIAL TABLET ON THE CAMPUS OF CORNELL UNIVERSITY

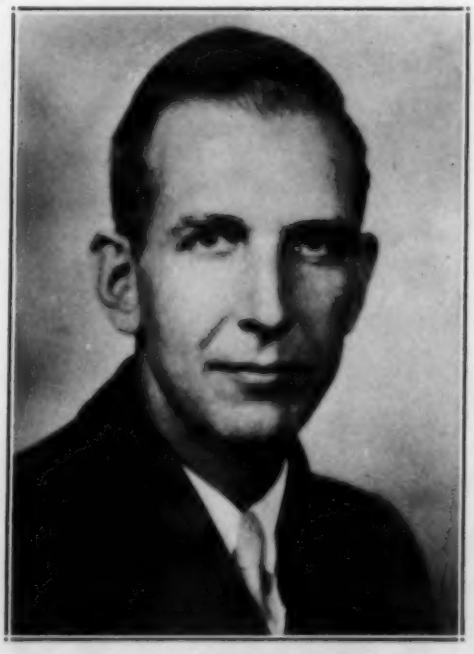
it in our lives. We can not have faith in the rationalization of life without seeking to promote it. In that effort we must not overlook the obvious because it is so simple."

A symposium on Saturday morning on the accomplishments and future of science, with Dr. Willis R. Whitney, vice-president of the General Electric Company, in charge of research, speaking for the physical sciences, and Professor Frank R. Lillie, of the University of Chicago, speaking for the biological sciences, brought agreement that by its very nature, the future of scientific discovery is essentially unpredictable. They agreed, however, that "without research there is no increase in knowledge and no progress in ideas," and that the lesson for society is to support free and independent research and to have abiding faith in the future results."

Dr. Whitney said:

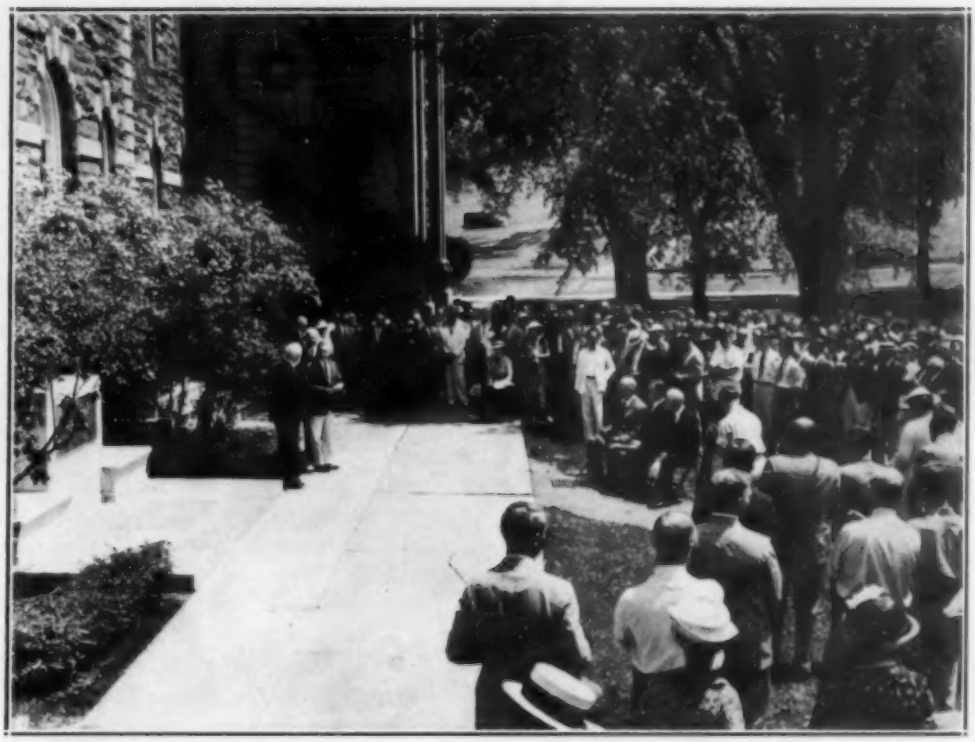
> There are no little or big discoveries to be made, but only true ones. . . . If industrial research were done by more and better people, we should have less unemployment, and perhaps none. . . . Research is the result of childlike inquisitiveness, and we are likely to check that inquisitiveness, as we do a child's, because we are lazy. . . . We almost need to provide more "accidents," for it is frequently the unexpected effect which drives a researcher into a new and productive field. . . . Scientific research is fishing with a tiny scoop-net in oceans of unfathomable depth and infinite area. We make the nets ourselves. Our catches often make a disordered pile, but we may orient them and fit them together as we do jigsaw puzzles. Then some one may give our puzzle one more dimension, like depth, and then we have a new necessity. . . . I doubt if it is recognized that the average research worker is a healthy, justified kind of unselfish communist. He aims at everything and works for everybody. He pays personally for the spread of his propaganda, and broadcasts freely all his home-grown produce. . . . The expense to a good worker may exceed several hundred dollars a year. The better he does it, the more it costs him. . . . When all is said and done, there is only one thing worth advancing, and that is the mentality of the man.

Sigma Xi semi-centennial research prizes of $1,000 each were presented, for work in the biological sciences to Dr. Richard E. Shope, of the Rockefeller Institute for Medical Research at Princeton, N. J., and for work in the physical



DR. RICHARD E. SHOPE

PROFESSOR ISIDOR I. RABI



THE PRESENTATION OF THE MEMORIAL TO PROVOST MANN OF CORNELL UNIVERSITY

sciences to Professor Isidor I. Rabi, of Columbia University. Eighty-five candidates were nominated by the chapters and clubs of Sigma Xi. In making the awards, Dr. Durand explained that the field of choice comprised "those already engaged in research and with notable work accomplished, but who might not be considered as yet having arrived at the full measure of their stature in science"; and that the awards were made "as a stimulus to research in progress and in the future, rather than as a reward for past and completed accomplishments." Dr. Shope's award is for work done on the etiology of swine influenza, particularly for determining the dual nature of the disease and thus establishing a principle which it is believed will have wide application in the control of infectious diseases. Dr. Rabi was recognized for his work on molecular beams and especially on the magnetic moments of the proton and deuteron, and because of the promise that this work holds for the future.

On Saturday morning, in the presence of three of the founders, Dr. Durand unveiled and presented to the university a memorial tablet, bearing the inscription:

The Society of the Sigma Xi
Devoted to Research in Science
Has Placed This Tablet Here
On its Fiftieth Anniversary
To Commemorate the Founding
At Cornell University
1886 1936

The tablet is set in a monument of Indiana limestone, erected by the university, with a stone bench at either side and a stone terrace before it. Set in a bay in the shrubbery near the southwest corner of Sibley College, it overlooks the main quadrangle. The bench was designed by Professor Alexander D. Sey-

mour, architect, and the tablet by Professor Harry P. Camden, architect. Accepting the memorial, Provost Albert R. Mann, who is president of Alpha Chapter of Sigma Xi, expressed the hope that "as one of Cornell's most treasured possessions, it will beckon countless generations of youth to a fellowship rich in its tradition and prophetic in its significance." L. C. B.

METEOROLOGICAL ASPECTS OF THE 1936 DROUGHT

At times of great national disasters caused by nature's vagaries there is a disposition to become panicky, to be convinced that the old order is changing and to believe that we, as a people, must adjust ourselves to a new régime. Many are expressing that thought in connection with the present drought, especially as it is the third that has occurred within so short a period as six years. Is there sound basis for such pessimism? Not if we can take past records as an indication of the future.

It is characteristic of precipitation records, in the long run, to vary in wave-like progression. That is, when short-period fluctuations are smoothed, there appear successive up-and-down trends, covering comparatively long periods of time and, in general, the difference in the rainfall for the phases of comparatively heavy and those of light amounts is marked. Such a tendency is shown, for example, in the records for the Great Plains, especially the central and northern portions. Thus, the normal annual precipitation for western Kansas is 19 inches. The 5-year average, 1902–06, was 22 inches and that for 1931–35 was 15 inches. Again, taking a single station, the average for 1891–95 at Dodge City, Kansas, was 16.5 inches, but for the following 4 years it was 25.5 inches, or more than 50 per cent. greater. Numerous other examples might be given. They all lead to the definite conclusion that the present succession of dry years is by no means unprecedented and that we may expect a return to normal and even above normal conditions. Unfortunately, we can not say as yet when that return will begin.

Optimism as to the future does not help very much, however, so far as the present situation is concerned. During this year, beginning with January, precipitation has been persistently scanty in most sections of the country. For the six months up to July 1 scarcely half the normal had been received in a considerable northwestern area and also in much of the interior, while nearly all sections between the Rocky and the Appalachian Mountains had less than three fourths normal. May and June, especially June, greatly intensified droughty conditions. For these two months the northern Great Plains had scarcely one third normal and less than half was the rule in the Ohio and middle Mississippi Valleys. It was the driest June of record in several states.

At this writing, July 10, the picture has changed somewhat in the South and East. During the first week in July generally heavy rains quite effectively broke the drought from Texas, Arkansas and southern Missouri northeastward to the Appalachian Mountains. However, from the Ohio River, central Missouri and northern Texas northward and northwestward only a few localities had received more than very light showers. This condition has been very greatly aggravated by abnormally high temperatures. The maxima have ranged well above 100°, even up to 120° in places, day after day since July 2, throughout a large area from the western Ohio and central Mississippi valleys northwestward. Unless rain and lower temperatures occur within the next two weeks, this drought will undoubtedly take its place as the worst in the history of this country and as one of the worst in the history of the world.

Willis Ray Gregg
Chief, U. S. Weather Bureau

EFFECT OF THE 1936 DROUGHT ON CROPS

THE weather of 1936, up to this writing, July 11, has been decidedly unfavorable for agriculture over the greater portion of the United States. Precipitation of the winter and early spring was very scanty in the Southwest, where the soil became extremely dry, and severe dust storms caused much damage by drifting and by blowing out winter wheat. However, in May there was a reaction to abnormally heavy rains, materially improving conditions; they were especially helpful to winter wheat over large areas, particularly in Kansas. These western May rains were largely responsible for the comparatively good showing of the 1936 winter wheat crop in the United States.

While May rainfall improved the outlook materially in the Southwest, other parts of the country were less fortunate, and droughty conditions slowly, but surely, developed. Dry weather in May and June brought wide-spread damage to early truck, hay and pastures, the latter becoming very poor in nearly all sections between the Appalachian and Rocky Mountains.

The spring was the driest of record in many southeastern sections, and great harm resulted to early crops in a considerable area, especially from North Carolina southward to central Alabama. It was too dry for cotton to germinate, and many spotted fields resulted. As before indicated, the winter wheat crop was not seriously affected by this year's drought, principally because of May rains in the western portion of the wheat belt, and comparatively cool weather in the eastern part, though it became decidedly dry and some deterioration resulted, especially in northern districts. The Northwest was entirely too dry for spring wheat, though up to the beginning of July temperatures had not been persistently high.

During the first week in July crops benefited materially from rain over much of the South, but rainfall was insufficient in Georgia and some adjacent sections. This month ushered in a period of extremely high temperatures in the mid-West and Northwest, which were disastrous to spring-sown small grains. The heat and drought seared what little pasture was left, and dried up available stock water. With neither feed nor water to be had over large areas, the live-stock situation became desperate, with heavy shipments necessary to prevent deaths by the thousands from starvation and thirst. The spring wheat crop and other small grains in the Northwest were severely damaged, but conditions continued favorable in the North Pacific area.

Up to July 1, corn had not been seriously harmed over wide areas, but the extremely high temperatures and lack of rainfall were beginning to be felt materially by the 10th of the month, with considerable irreparable damage in evidence by that date, especially in the southwestern Corn Belt. At this writing, July 11, much of the corn crop is entering a critical stage of growth, and it is certain that, if high temperatures and dryness continue for an appreciable time, there will be a major disaster to this important American product. The fate of the corn crop will be decided largely by the weather of the two weeks succeeding July 11.

J. B. KINCER
CHIEF, DIVISION OF CLIMATE AND CROP WEATHER, U. S. WEATHER BUREAU

ACTION OF FLUORIDES UPON HUMAN TEETH

MOTTLED enamel, or what is now known scientifically as chronic endemic dental fluorosis, is a water-borne disease associated with the use of domestic waters containing toxic amounts of fluorides. The toxic action of the fluor-

ides upon the teeth is apparently operative only during the period of dental calcification; consequently the susceptible period for the human is between birth and eight years of age. The second, or permanent dentition, is particularly affected.

Normally calcified teeth erupt, presenting a smooth, glossy, translucent structure, generally of a pale creamy white color. Teeth affected with mottled enamel, on the contrary, erupt, showing a dull, chalky-white appearance which in many instances later take on a characteristic brown stain, the frequency of brown stain increasing with age. In areas where the water contains a relatively high fluoride concentration the teeth may in addition be marked by discrete or even confluent pitting. As the defect is structural (an endemic dental hypoplasia), the result is a permanent physical disfigurement of thousands of children residing in endemic areas during the period of tooth formation.

At present there are approximately 335 surveyed or reported endemic areas in this country alone distributed among 25 states. Because of the high incidence and its wide-spread distribution, it is a public health problem of country-wide interest and particularly appropriate for study by the national health service. Although the objective sign of endemic fluorosis is dental, this disease may be one of multiple ramifications. It is becoming one of increasing interest to physicians, especially pediatricians, epidemiologists, sanitary engineers, water chemists, biochemists, geologists and veterinarians.

Studies by the United States Public Health Service during the past few years, in addition to largely augmenting the number of known endemic areas, have demonstrated the importance of studying this problem from a quantitative point of view. In this connection there has been developed a quantitative classification for clinical diagnosis and a standard method for determining a community mottled enamel index. It has also been shown that there is a definite quantitative relation between the fluoride content of the communal water supply, the incidence of affection and the percentage distribution of severity, particularly the latter. Earlier studies at the National Institute of Health, in addition to confirming the findings of earlier workers regarding the fluorine hypothesis, indicated that a given concentration of sodium fluoride in the drinking water produced a more toxic reaction than the same concentration of sodium fluoride in the diet. Carefully controlled epidemiological studies are being carried on at present for the purpose of obtaining definite information concerning the maximum amount of fluoride permissible in a domestic water supply. Studies to date indicate that amounts not exceeding one part per million are of no public health significance.

In the light of present knowledge this disease is readily preventable if water free of toxic amounts of fluorides is used during the susceptible period for both drinking and cooking. Several small communities have actually changed their public water supply to one free of toxic amounts of fluorides in order to prevent the further development of this defect. In many endemic areas, however, this mode of action is not feasible and final solution will no doubt be dependent upon the development of an economical chemical method of treating the present supply.

H. Trendley Dean
Dental Surgeon
U. S. Public Health Service

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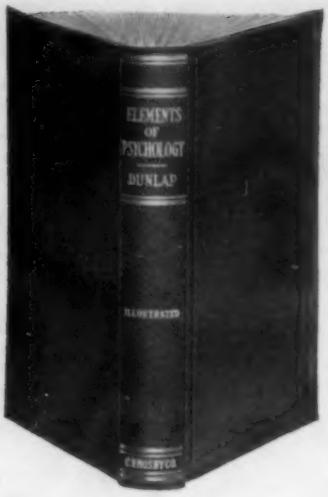

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